

**THE EARLY THEORETICAL DEVELOPMENT OF
ALEXANDER ROMANOVICH LURIA**

An Exploration of His Work 1921-1936

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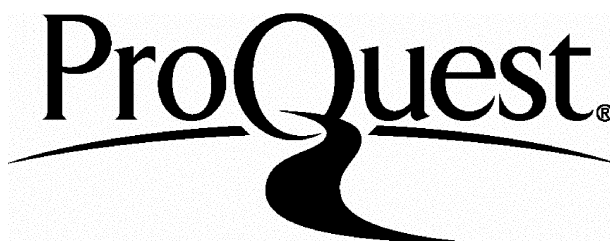
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MICHAEL PAUL GEORGE HAMES

ABSTRACT: Alexander Luria (1902-1977) is famous as a founder of neuropsychology, but his early theoretical development has never been seriously investigated at any length.

Part I, *The Early Years*, deals chronologically with Luria's development from 1921-6. It looks at his intellectual background, his early experiments using his combined verbal and motor response method of investigating the structural dynamics of stress. It examines his use of objective approaches to reflexes in Pavlov, and his attempt to combine it with Freud's psychodynamic approach. Luria's early collaboration with Lev Vygotsky is explored, together with their joint and individual attempts to resolve the apparent methodological impasse this combination presented to explaining the nature of higher psychological processes.

Part II, *The Nature of Human Conflicts*, looks at the liberating effects of *Gestalt* theory on their thinking, together with their criticisms of it. It concentrates on Luria's series of experiments up to 1930, and how his development of the 'functional systems' approach resolved many of the problems. Luria thereby provided the neuropsychological basis for much of Vygotsky's approach.

Part III, *Cultural-Historical Theory*, is short. It looks at some of the origins of the theory and the attacks on it, together with some of the reasoning behind Luria's expeditions to Central Asia to compare modes of perception and thinking in different forms of society.

The overall theme of this thesis is how Luria and Vygotsky struggled to explain the developing role of higher psychological functions on an objective experimental basis, and how in the course of ontogenetic development the neuropsychological organization of cognitive functions and human behaviour is dialectically transformed. This process explained how phylogenetic, ontogenetic, social and historical factors were integrated in the course of development. It therefore also potentially allowed for the methodological integration of psychology into a unified science.

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Alexander Romanovich Luria (1902-1977)
Chronology of Major Events and Publications 1917-37

1914-1918

World War I

1917

February and October Revolutions

1918-21

Russia invaded by many foreign armies and counter-revolutionary armies
 financed by the West

Luria studied at the University of Kazan

1920

Beyond the Pleasure Principle (Freud)

1921

Russia introduces the New Economic Policy to help recovery

The Mentality of Apes (Koehler)

1922

Luria founded the Kazan Psychoanalytic Society

1923

Luria's first booklet published

Luria moved to Moscow and married Vera Nikolaievna Blagidova

Luria became secretary of the Institute of Psychology and head
 of its experimental laboratory

He also became secretary of the All-Russian Psychoanalytic Society

*Twenty Years' Experience in the Objective Study of the Higher Nervous Activity
 of Animals* (Pavlov)

1924

Lenin died. Stalin moved into a powerful position

Trotsky became increasingly marginalized

Luria placed in charge of the sub-faculty of psychology and its laboratory at the
 Communist Academy of Education

He began his celebrated series of experiments into affect, conflict and will

Vygotsky lectured in Petrograd and joined the Moscow Institute of Psychology

The German Ideology Part I (Marx & Engels)

1925

Kurt Goldstein published major articles on neuropsychology

The Dialectics of Nature (Engels)

Psychoanalysis as a System of Monistic Psychology (Luria)

Consciousness as a Problem for the Psychology of Behaviour (Vygotsky)

1926

Intention, Will and Need (Lewin)

The Methods of Reflexological and Psychological Investigation (Vygotsky)

Luria's writings began to show the influence of the ideas of Goldstein and Lewin

1927

Trotsky sent into 'internal exile'

Luria resigned as secretary of the Psychoanalytic Society

Vygotsky and Luria began to formulate ideas on Cultural-Historical Theory

1928

Luria developed his initial version of the concept of functional systems

1929

Stalin expelled Trotsky from Russia. He started the first Five-Year Plan and began to introduce the system bearing his name, which included the oppression and exploitation of the workers and peasants, the elimination of opposition and independent thinking

Luria met members of the Gestalt school in Berlin

He then met other leading members of the international psychology community at the international conference in New Haven where he presented two papers

He found a publisher for his research in America and handed over the completed part of the text

His first marriage was dissolved

1931

Luria led the first expedition to Central Asia to compare the cognitive and perceptual behaviour of culturally and socially different groups

He was strongly criticized on his return and increasingly hostile attacks were made on Cultural-Historical Theory

Vygotsky's team was broken up, and several leading members, including Luria, relocated to the Ukraine to work in the Kharkov Psycho-Neurological Institute of Medicine

1932

Luria led the second expedition to Central Asia

He was again attacked and banned from organizing a third expedition

The Nature of Human Conflicts (Luria)

1933

Hitler gained power in Germany

Famine in the Ukraine as a result of Stalin's agricultural policy

Luria was warned to keep a low profile and keep out of Moscow, because of the threat of arrest

Luria married Lana Pimenovna Linchina (1904-78)

1934

Luria returned to Moscow full-time and worked with Solomon Levit at the Medico-Genetic Institute studying twins

Death of Vygotsky

Thinking and Speech (Vygotsky)

1936

Decree on Pedology in effect makes most of child psychology unacceptable

Luria resigns his two posts and becomes a full time medical student

1937

Execution of major psychologists barely noticed among the mass of victims of Stalin's purges

Preface

I

Problems in the Investigation of the Early Work of Alexander Luria

There are several monographs devoted to the work of the Russian psychologist and neuropsychologist Alexander Romanovich Luria (1902-1977). Most of them are devoted to various aspects of his work in neuropsychology. There are also collections of papers dedicated to his work, together with biographies and contributions towards a biography together with autobiographical works. There are however no works that attempt to look at the period of his early career in psychology (1921-1936) as a whole, nor does any detailed work exist that attempts to make sense of this period as a whole. This is surprising given that when Stephen Toulmin (1978) dubbed Vygotsky “the Mozart of psychology” he also described Luria as its “Beethoven”. The period of their interaction was important - a view that Luria expressed many times – both for the results of their interaction and the development of Luria’s own ideas.

At first this preface looks at some of the problems involved in examining this phase of Luria’s career. It does so by focusing on issues that arise almost spontaneously from reading his autobiography, *The Making of Mind*. This book has proved invaluable, because it provides useful information and is the only one to include coverage of this whole period, though important episodes are omitted. One of Luria’s editors, Michael Cole, however, made plain in the introduction and epilogue that the work presented difficulties. Here I have made comments arising from it to indicate the range and significance of the problematic areas relevant to this thesis.

Most obvious is the startlingly few references to Luria’s family, or to the problems he faced as a scientist after Stalin overthrew the ‘Old Bolsheviks’ and introduced what I think is best described as his state-capitalist regime, which by 1936 effectively prevented the functioning of psychology as an independent science. Even after Stalin’s death in 1953 his legacy remained and Luria was compelled to politically censor his comments up until his death (Cole 1992, personal communication).

Elena Alexandrovna Luria's posthumously published 1994 biography of her father provides further information, especially on certain aspects of the family, and the impact of Stalin's repressions on it. Her mother told Michael Cole that Vygotsky went to Luria's father and warned him to get his son out of sight (Cole 1992, personal communication). Certainly Luria was under great risk of arrest in 1933-4 (E. Luria 1994, chapters 5 and 6). Yet, his enemies alleged that, even as late as 1936, Luria was prepared to orally defend the theoretical views for which he had been subjected to a witch hunt in the early 1930s (G.F. 1936, cited in E. Luria 1994, 75). Luria recognized that the continuing attacks on psychologists including himself meant that it was impossible to continue in psychology and he resigned his posts in December 1936 to become a full-time student at the First Moscow Institute of Medicine. Although this was one logical development of the work he had been doing throughout the mid-1930s, he clearly had no other choice. He was not alone, but his timing saved his life. His close friends and colleagues Isai Sapir, Solomon Levit, and Isaak Shpilrein were arrested (Levit in January 1937) and subsequently executed. This provided a clear signal from the state that independent views and those that continued to argue for them would be eliminated. Elena Luria's account suggests that her father survived by the skin of his teeth, because he moved to an inconspicuous position at the time when he did (ibid. 75-6).

It is at that point that my thesis ends, but the pressures on Luria remained and explain why he had to self-censor. We have recently learned that Luria's brother-in-law was arrested in 1937, and his sister, Lidiia Romanovna, was eventually rescued from the prison camps only by her father's intervention. Roman Albertovich Luria held a position as a Kremlin doctor specializing in gastric disorders. Fortunately the gastric pains of the Prosecutor General, Vyshinsky, were such as to allow Roman Al'bertovich to persuade him to commute Lidiia's sentence to exile (Levitin 1998, II, 41-2). In 1953 when Stalin's paranoia produced the so-called 'Jewish doctors' plot' (cf. Rapoport 1991), "Alexander Romanovich expected to be arrested from one day to the next, and so kept a small suitcase ready containing his necessities" (E. Luria 1994, 146).

But while Luria's self-censorship consisted of omitting information about his family and politics, and only publishing certain of his researches and those of Vygotsky when he felt it was safe to do so, his self-effacing approach presents bigger difficulties for the period before 1936. He concluded his autobiography with the following comment, "People come and go, but the creative sources of great historical events and the important ideas and deeds remain. That is perhaps the only excuse I had for writing this book" (Luria 1979a, 188).

This approach, that of the self-effacing scientist at the service of humankind, was consistent with his long-held beliefs. It accounts for some of the opaqueness that most readers find in the book. Luria's death prevented the completion of the checking of the text, and his anticipation of it may have led him to concentrate on important scientific developments in psychological thinking without sufficient explanation of their genesis – or of his specific role in their genesis.

He had written just prior to the above conclusion, "There is no [biographical] subject of exceptional abilities – I have none. ... But there is the atmosphere of a life, beginning at that unique time which was the start of the Revolution. There is a period of exploration, the meeting with a genius [i.e., Vygotsky] and falling under his influence, and the series of deeds that a scholar could accomplish during a rather long life" (Luria 1979a, 187-8). While here Luria praises the creative surge that accompanied the Bolshevik Revolution of 1917, (with the implicit comparison of the repression of both Tsarism and Stalinism), his own role remains unarticulated. Thus it is not apparent how his role meshed, interacted or contrasted with his collaborators.

One of Luria's major collaborators in the second half of his career, E.D. Khomskaia, writes of his modesty, avoidance of disputes, and complete lack of vanity. He avoided professional advancement in order to remain active at his experimentally creative work (see Homskaya 2001, Introduction and Conclusion). As a consequence, "he spoke rather modestly of himself and the epoch, not mentioning the difficulties he had to overcome". It is "evident that Luria, being free of all vanity, underestimated the scale of his personality and

contribution to international psychological science” (ibid. 113). There are numerous examples, and additional reasons for his behaviour.

Cole (1979, 189) relates how he could rarely persuade Luria to talk about the social and personal context of his work. Nor could he get him to seriously discuss his early work. He continually played it down while at the same time reiterating that Vygotsky was a genius. Oddly enough Cole notes (ibid. 224) that Luria “could no longer tell me why the man had so excited him.” Perhaps this may be excused in that Luria was a man in his mid seventies with a heart condition that was about to kill him, whose wife was also dying from an incurable and painful illness.

Luria, in a lecture in 1974, discussed briefly his first published book, *Psychoanalysis in the Light of the Main Trends in Contemporary Psychology*, written at the age of 20 in 1923. He describes its origin in sufficient detail to preclude any question of memory loss. He tells how he took galley proofs of “a review of books on psychoanalysis” he had written “and made a book of them” (cited in Levitin 1982, 152-3). There is no way that this precocious methodological analysis and comparison of aspects of psychoanalysis and contemporary trends in psychology could be described merely as a collection of book reviews! Here it could be assumed that he was simplifying in order to entertain his audience of students.

In *The Making of Mind* Luria writes of the use of the seminal concept of ‘functional systems’ in relation to the work of P.K. Anokhin and N.A. Bernshtein. He writes that the term was introduced and developed by Anokhin in 1935 (Luria 1979a, 124), yet Luria used it before 1930. As we shall see Anokhin and Bernshtein’s definitions of functional systems were different in emphasis to Luria’s and involved different elements and concepts, which are equally important. I assume Luria used Anokhin’s version initially to promote the concept, but also to seek some form of cover from attack behind a purely physiological-neurological version. This also promoted the important but unjustly neglected works of his colleagues, one of whom, Anokhin, was a lifelong friend.

Luria then stuck to this version of its origins. It does not help clarify Luria's own role however.

At the end of a chapter on the work Luria undertook for what eventually became his *doktorat*, he wrote that the application of the techniques and approaches that he used “did not lead to a basic reconstruction of psychology as a science. That enormous task, which was beyond my limited capacities, presented itself to me quite unexpectedly in 1924. In that year I met Lev Semenovich Vygotsky. This event was a turning point in my life as well as in the lives of my colleagues in Soviet psychology” (ibid. 37).

This passage crystallizes several key issues. The dramatic effect of Vygotsky on his contemporaries is unquestioned. His daughter's 1996 biography of him gathers convincing testimony of his charismatic appeal and lasting influence. But we must beware of investing him with mythic powers. It is evident that the overwhelming part of Luria's *doktorat* was carried out independently of Vygotsky. Two of the latter's biographers, Rene Van der Veer and Jaan Valsiner (1991, 184), go to the extreme of suggesting that the collaboration of Vygotsky and Luria only became serious in 1928 or 1929. Certainly they often worked in different areas. Iaroshevskii dubs Vygotsky's first year in Moscow as the “defectological year” (Yaroshevsky 1989, 121), because of his concentration on a new field, which involved the education of the deaf and dumb. On his return from an international conference on the subject, which took place in London in June 1925, Vygotsky suffered a bad attack of tuberculosis. Prevented thereby from working at the Institute of (Experimental) Psychology, he then wrote his work, *The Psychology of Art* (ibid. 139-40). He was more seriously ill in 1926. But we know that in spite of this he co-wrote two prefaces with Luria published in 1925 and 1926. And Van der Veer and Valsiner (1991, 13) themselves cite a letter written by Vygotsky to Luria, dated March 5, 1926. Vygotsky says, “I very much deplore the fact that in this difficult time of crisis I am not with you at the Institute... How seriously we have to think about our [scientific] fate and the fate of the cause that we undertook, when K.N. [Kornilov] and the other bosses do not wish to think about it”. Unfortunately we are not informed what this ‘cause’ was.

Both Luria and Vygotsky were working to produce a psychology that could lead to a comprehensive understanding of the individual, and both tried to envisage systems by which this could be achieved – whether biologically, physiologically, neurologically, or in terms of society and the role of language – and ultimately a combination of all these approaches. And it was not until the late 1920s that they both saw that a major part of the solution to these systemic complexities lay in the issues involved in development.

The necessity for a change in the methodological approaches of psychology was evident to both Luria and Vygotsky even before they met. This was surely a major factor in Luria's thinking when he ensured that Vygotsky was recruited to his department in 1924. How they succeeded in producing these methodological developments by 1930 is a major concern here. They certainly collaborated in some tasks from the very beginning (cf. Radzikhovskii and Khomskaia 1981). No one disputes Vygotsky's influence on Luria, but precisely how this was effected before 1929 has rarely been investigated – and certainly not at length. Equally relevant, but as yet uninvestigated – indeed barely even raised (ibid. 9, n.1) – is the question of whether and, if so, how Luria might have influenced Vygotsky. By and large, Luria's playing down of his own early work, coupled with his praise for Vygotsky, has ensured that writers on Vygotsky have tended to present the latter as initiating and arranging virtually everything and Luria as acting largely as a transmission belt and as a major organizer of experiments. Although this *may* be convenient for some writers on Vygotsky, a writer investigating Luria's early work can hardly accept this as a *fait accompli* or simply focus on those works that Luria researched independently. For even the notionally independent works must willy-nilly exhibit the effects of interaction: *The Nature of Human Conflicts*, the 1932 American translation of Luria's research, does share quite closely some of Vygotsky's concerns in some of the later research. Similarly some of the earlier experiments parallel some of Vygotsky's theoretical lectures and writings. The use of the concept of 'functional systems' is just one indication that there are major theoretical advances implicit in the work, *despite* Luria's remarks to the contrary. It is impossible to dispute Khomskaia's view that "during these early years (1924-1934; the years of collaboration with Vygotsky),

Luria actively participated in both the practical and theoretical birth of the new science” (Homskey 2001, 83).

As Vygotsky and Luria jointly and singly grappled with the methodological issues and their concrete application, the advances *and* the problems of one would be of concern, interest and stimulation to both. This interaction is known in some circumstances, and I make no apology for raising the possibility in others. One problem with presenting these parallels and possible interactions is that the evidence provided here comes mainly from the published articles. It is not possible to say who got one particular idea first, because, although the articles may derive from earlier lectures, new ideas may have been added later. Similarly, delays in publication and unreliable publication dates are confusing factors. In *The Making of Mind*, Luria presents the arrival of Vygotsky after having already discussed most of the work for his *doktorat*, even though Vygotsky arrived just after it was begun. Here I have tried to illuminate the parallels and presumed interactions of Luria and Vygotsky. Although this has the merit of allowing this to be shown, and thus improving the historical accuracy of their collaboration and development, there is inevitably some overlapping and duplication of arguments in their separate papers – for example in Chapter Four. And given the caveats I have made concerning the articles, the precise chronology of their interaction is at best imperfect. It is, however, important to examine this. Ultimately the questions about influences may only be answered when Vygotsky’s correspondence and Luria’s diaries and correspondence become available. While my suggestions are provisional, I do think that both my approach and the framework of this thesis help in posing the questions that should be asked.

I feel that many writers, notably those who are not writing about Luria specifically, are quick to make judgments or assumptions about Luria’s work. One feels obliged to answer these points even though many of them might appear sectarian. In order to counter this problem, and to draw those concerned and interested in Luria into the discussion, I have adopted an exploratory approach. I have tried to raise or speculate about relevant issues, so that if the reader wishes to disagree with my conclusions, he or she can then investigate other options. In the course of the work I do occasionally draw conclusions that are initially

plausible within the context, but are subsequently shown to be partial. Some conclusions remain provisional or are only arrived at on the balance of what we know. Within reason this seems a sensible course of action. The exploratory approach has I feel also been a necessary one. Only with the completion of this thesis is it now possible for me to envisage a more conventional and traditional approach to giving an account of Luria's theoretical development.

The focus on methodology explains the famously varied range of research by Luria in this phase of his career. "Every investigation proceeds in cycles, and the attempt to complete one cycle is at the same time an attempt to begin planning the problems of subsequent researches"(Luria 1932a, 169). He may have labelled his early works 'pilot studies', but he certainly considered them all significant enough to write about. The thread connecting these works is the attempt to study the systems of human behaviour as a whole and in their many aspects and relationships, together with the methodological implications and practical experiments this entailed. The practical experience and learning involved in this process provided an invaluable foundation for Luria's subsequent development, and one that proved extremely useful for both Luria and Vygotsky in the development of their theories and in the organization of their investigations. The construction of a methodology that could be applied to psychology as a whole science, as opposed to a set of discrete disciplines, was a crucial element of Vygotsky's work. But also, and like Luria, he was a practical psychologist. Grounding psychology on a materialist basis that extended throughout the science was his chief concern in the 1920s.

In some respects the tasks of this thesis are made easier today by the accessibility of Luria's writings, including his autobiographical writings, together with the biographies of others. The translations of Vygotsky and the biographies of him are a major source of information, as are the many other writings of and on the period and its leading actors. It certainly makes it easier to raise the issues previously mentioned. Indeed this is one of the major aims, and I hope it will prove useful to investigators if and when the letters, diaries and texts become fully accessible. In turn they will hopefully then be able to offer a more authoritative resolution to these questions. Luria's *The Nature of Human*

Conflicts, his major publication of the time, was, his contemporaries considered, so poorly translated that it is not easy to fully grasp the importance of this period of Luria's work (cf. VI, i, 157; VII, iii, 212). As I write, a Russian edition of the book, based on the Russian manuscript, is being prepared for publication. But there are many other deserving candidates for new editions. It is also well known that of the material that Luria collected on his Central Asian expeditions of 1931 and 1932, only a quarter was published. A Russian publication appeared only in 1974 - after Luria was persuaded that it was safe enough to do so - when he published his account, *Cognitive Development: Its Cultural and Social Foundations* (1976). There have in the recent past been problems with access to Luria's personal archives (cf. Levitin 1998, 2, 45), but there now seems to be an improvement. This thesis and its questions will therefore hopefully contribute towards this ongoing process of learning and re-evaluation.

The other major aim is to show the underlying unity of Luria's work in this period, both in terms of practice and methodology, and how this work developed. I agree with Luria's conclusion about the earliest period of his work in Kazan up to 1923. Luria (1979a, 27) writes, "Throughout this period of my life I was naively groping. Still, after fifty years, I have the feeling that many of these activities were significant in my further development as a psychologist. In later years the surface appearance of my research changed a great deal. But the central themes that had guided my initial efforts remained". Thus Luria clearly considered this period before he met Vygotsky as vital for his subsequent work, as well as the period after he met Vygotsky.

II

The Organization of the Thesis

In practice the writing of this thesis was made easier by the fact that the long-standing 'crisis in psychology' meant that Luria, like Vygotsky, had to come to grips with the strengths and weaknesses of key approaches to psychology in general. This provides, therefore, a contribution, though a partial one, to the 'history of ideas' in psychology. Luria's theoretical and methodological response and development were based on working his way through and beyond such

approaches. This provides a framework of development – one common to many – though of course the solutions varied. I was surprised at the extent of the ferment of ideas that existed at the very beginning of this period. The discussion and clarification of some of these ideas fortuitously served the purpose of priming the reader to some extent for certain later discussions.

We tend to assume that the work of Pavlov and others was an intrinsic part of Luria and Vygotsky's scientific background, providing both with a start on their materialist basis, and a provocation to them to criticize him and his assumptions. This is of course true, but we must remember that the collection of Pavlov's lectures on conditional reflexes was first published as late as 1923. Vygotsky and Luria's responses to this and subsequent editions of Pavlov's work (Pavlov 1928) show how much this continued to stimulate them and improved the materialist basis not only for their individual work, but also for their collaboration.

Part I of the thesis, *The Early Years* (1921-1926), finds Luria, and Vygotsky, attempting to both use, and find a way past, reflexology and psychoanalysis. The first four chapters deal at length with these problems. This is important for allowing us to see how they singly and jointly attempted to deal with this. Despite being critical of certain elements within these schools from the very beginning, and increasingly so over this period, they still found themselves to some extent dependent on these schools and, though wishing to break free, being, in a sense, still restrained.

Part II, *The Nature of Human Conflicts*, abandons a strict chronological approach in order to deal with the research that was eventually published in 1932 under this title. However, the first chapter, Chapter 5, deals with the enormous effect of Luria's contact with Gestalt theory in showing him, and Vygotsky, ways out of their previous dilemmas. Chapters 6 and 7 deal mainly with the experimental research that went into the book and the new theoretical approaches this entailed. Here I concentrate almost entirely on Luria in order to bring out his ideas. For, in this period, he developed major theoretical approaches.

The existing translation of *The Nature of Human Conflicts*, on which these chapters are based, is, as previously mentioned, so poor that it needed both revising and ‘reinterpreting’ to bring out Luria’s emerging theory. Therefore I make no apology for the extensive quotation of this ‘retranslation’. Without it, it is difficult to understand the major changes in Luria’s thinking. Since it is a ‘retranslation’ I feel obliged to present my version of Luria’s words to the reader particularly in what I think are the significant areas. I hope the selection of quotations itself shows how Luria’s theoretical ideas developed. It is rather unusual both to ‘retranslate’ and thus re-present a work in a different light to the extent that I have done here, but it is both a necessary and valid part of the history and philosophy of psychology. Only when this work is made accessible and comprehensible does it become possible to assess and appreciate its full significance.

I do, of course, add further elaboration and clarification throughout. An assessment of contemporary reviews of the book is given in the final section of Chapter 7, which also includes a discussion on the various forms of the concept of functional systems and their development in this period.

Parts I and II form the major part of the work. They are so to speak the foundation stones for everything that follows in Luria’s career. Part III is something of an epilogue, but it is labelled as a ‘Part’ in order to draw attention to the fact that it deserves to be given far more weight. This deficit, which I hope to address elsewhere, is due to word limitations. I felt it was more important to concentrate on the earlier period of Luria’s development and his own research, not only because they are insufficiently discussed, if at all, but also because they have both general theoretical significance and because they ground his other work. Part III, ideally would discuss Luria’s role in the Vygotsky team’s investigation of child development, and his own contributions to the field. It would discuss at length his expeditions to Central Asia to compare cognitive approaches in different forms of society, and finally it would look at Luria’s comparisons of child development in twins.

As it is, the space available for Part III, *Cultural-historical Theory*, limits the options considerably. The change in Russia's political, social, economic and academic climate is briefly addressed. I attempt to explain the reasoning behind Vygotsky and Luria's development of cultural-historical theory. Usually this crucial theoretical aspect gets lost in the detail of the investigations. Here unfortunately there is barely room even for a sketch of Luria's expeditions to Central Asia in 1931 and 1932, but the theoretical contribution will, I hope, offer the opportunity to step back and look at the theory as a whole. Appendix 1 offers an account of how the ideas of Vygotsky and Luria on language and socio-historical development originated in the ideas of Johann Gottfried Herder (1744-1803), and these were taken up earlier by others, including Karl Marx. It involves the recognition that in this respect Vygotsky and Luria here form part of a much wider, though not adequately understood, historical school. I hope thereby to remedy what I see as a large omission in our understanding of Vygotsky. The Appendix has considerable bearing on the contents of Part III and the work as a whole.

There is insufficient space for an overall bibliography of Luria works, only for those works referred to. The most comprehensive bibliography is to be found in Homskaya 2001, but the transliteration is in an appalling state and there are many mistakes and omissions. Elliger and Scheerer's 1980 bibliography of works in English, French and German is still useful for this period. Works mentioned in my references are only those quoted and those mentioned in respect of further reading in psychological areas.

The thesis shows Luria's development, but it is only strictly chronological where possible and/or desirable. It therefore requires both some priming of the reader regarding specific questions, and some cross-referencing as part of the on-going commentary. In order to avoid confusion with the various works by Luria that are analysed here, together with *their* parts and *their* chapters, I have adopted a special form for my own cross-references. I have changed the chapter and section headings to all Roman numbers – e.g. (cf. III, ii). Where page numbers are also relevant these cross-references are presented as e.g. (cf. III, ii, 74). (The 'Contents' page provides chapter headings and the titles of their subsections). I

have avoided using endnotes except in Appendix 1. I believe that including relevant comments in the text does not, by and large, affect readability. All square brackets contain my own comments or additions. Occasionally I have added my initials to emphasize a comment of mine. All italicized words in quotations are in the original unless stated otherwise.

I have chosen to include a lot of quotation from Luria and works that influenced him directly, or that I think are relevant. This is important, because this has not been done before and because it is crucial not only to understand Luria's development, but also to demonstrate it. In sections devoted to particular works or chapters by Luria, particularly from *The Nature of Human Conflicts*, I have often simply used the page numbers in the references. In general, in a case where several quotations from the same page are used in sequence, the reference will be found in the final quotation. Where possible I have checked or made my own translations of the German texts cited. I have also made a very few short translations from the Russian. Although I readily admit the limitations of my own abilities in Russian, they have however given me an insight into the limitations of other translations from the Russian in this period. These generally relate to the inability of the reader to comprehend their meaning in English! Therefore I have freely amended such translations. Fortunately Nadege Renaud has allowed me to use her typescripts of some of Luria's early untranslated works (Luria 1923a, 1926c, 1932b), and to read parts of her translation of Elena Luria's biography of her father (E. Luria 1994). Although in Russian Luria is credited with having an elegant style, this is rarely evident in English translations. Nadege Renaud provides a model of how it should be done.

For the transliteration of Russian names and publications I have generally adopted the Library of Congress scheme, though I have omitted diacritical marks. I have however kept to some standard transliterations in a few well-known cases, including the subject of the thesis. Alexander Romanovich Luria should be rendered Aleksandr Romanovich Luriia, but since Luria is an international Jewish name, and Alexander also a standard transliteration, I feel this is acceptable. In general, in the text, I have also included Russian patronymics, or at least full initials, when introducing their bearers.

Among those who have answered queries or provided articles I would like to thank Konstantin Anokhin, Alan Clarke, Ann Clarke, Michael Cole, Nigel Foreman, Nadia Kerecuk, Alex Kozulin, Monika Niemann, the late Eckart Scheerer, the late Brian Simon, Joan Simon, Gregory Walker, and Sheila Zangwill. I would like also to thank Nadege Renaud for the use of her typescript translations. Thanks also to the staff of the Inter Library Loans Service, The University of London Library, the various libraries of University College London, The School of Slavonic and East European Studies, the Institute of Neurology, the Institute of Education, the Institute of Germanic Studies, Goldsmiths College and the London School of Economics. I would also like to thank the staff at Westminster University, in particular the late and truly missed Alla Figoff, for attempting to teach me Russian. I also thank Peter Hames and Nadia Kerecuk for reading early drafts of Appendix 1. I am, of course, responsible for the content, the analysis and the approach of the thesis. Also, since in my generation it was not the custom to receive verbal encouragement beyond primary school, I would like to thank those who did give encouragement to my academic ambitions over the decades, especially David Jones, Laurence Davies, and Brian Morris.

Finally, heartfelt thanks are due to my wife, Anne Hames, and to my supervisor, Richard Rawles. Their long and helpful support has been shown in many ways and was indispensable. In recognition of this I would like to dedicate this thesis to them.

PART I

THE EARLY YEARS

Chapter 1: Kazan

I

Family, University Studies, & Early Influences

Alexander Romanovich Luria was born on 16 July 1902 in Kazan, a town near the Volga, several hundred miles east of Moscow. He was brought up as a member of a professional non-religious Jewish family. His father, Roman Al'bertovich Luria (1874-1944), together with his three brothers, received a higher education in Russia, despite the quota system, which discriminated against those of Jewish origin. Though too poor to own an overcoat, he nevertheless completed his medical studies and graduated from Kazan University in 1897 and also studied in Germany. Alexander and his sister Lidiia, born in 1908, were brought up in a family where German was a "second native language". Evgeniia Viktorovna nee Haskin (died 1950), Alexander Romanovich's mother, was the daughter of a master watchmaker. She qualified as a dentist in Warsaw, then part of the Russian empire, and formed her own private practice. From this she helped to finance her husband's scientific career. Although he had been offered a post in a university pathology laboratory, the Ministry would not allow it, because of his ethnic origin. He defended his doctorate in the year of Alexander's birth. The subject was 'Sensory Nerves and the Diaphragm in the Innervation of Breathing'. Although he gained no significant university work until much later, his private practice was renowned throughout Kazan. Alexander loved both his parents, though I would be surprised if he was not just a little in awe of his father (based upon E. Luria 1994, chapter 1).

After the Bolshevik revolution of 1917 the discriminatory barriers were removed and Roman Al'bertovich's career took off. His son Alexander described his family as typical of the Russian *intelligentsiia*. "We considered ourselves progressive and had no religious traditions. Although we were sympathetic to the revolutionary movement, we were not directly involved in it" (Luria 1979a, 18).

Roman Al'bertovich became professor of Advanced Medical Training in Kazan in 1920, and then in Moscow in 1930. He became a Kremlin doctor in the 1930s. "His fundamental work was dedicated to the question of the pathology of the

digestive system, which he treated from the perspective of I.P. Pavlov's studies" (Vvedenskii 1954, vol. 25, 479). Luria was also concerned with the effects of malaria and syphilis on the internal organs. He became well known as one of the first to study the role of the psyche in the origin and course of internal diseases (cf. the references in *ibid.* and E. Luria 1994, Homskaya 2001. Cf. also the article by R. A. Luria 1987, and a review by Sagal 1944).

He naturally hoped and expected that his son would follow in his footsteps and was appalled when Alexander Romanovich became involved with the "non-scientific" field of psychology. The younger Luria was well aware of his father's standing in the medical profession. One assumes he was glad to investigate different fields from his father, and the spirit of the times naturally encouraged this. On the other hand, Luria's later comment would seem to apply almost from the very beginning: "I have always leaned toward clinical and physiological psychology" (cited in Cole and Cole 1971, 88). And the effects of such an upbringing must surely have been profound - not simply on Alexander Romanovich's scientific application and concern with methodology, but also on the prodigious work-rate for which he was famous throughout his long career.

He studied at Kazan University from 1918 to 1921 and obtained a degree in "Humanities" (Luria 1974, 254). The dramatic changes following the Revolution meant that the content of some courses continually changed. Luria says that he joined the Law department, but that it quickly became the department of Social Sciences (cited in Levitin 1982, 150). Sometimes the teaching staff left, in other cases they changed their courses. Luria (1979a, 17) notes his lack of a systematic education at university, but, like others, was invigorated by the many creative changes.

Luria joined in enthusiastically in the many student discussions, notably of what he terms "utopian socialism" – as a youth Luria "considered himself a follower of Tolstoy" (Cole 1979, 199). "Under these conditions student discussion and student initiated projects soon came to dominate the professors' lectures" (Luria 1979a, 20). Luria became president of a student society, the Association of Social Sciences, which was wealthy enough to publish its own books.

He was interested in the history of social thought and considered writing a trilogy on how ideas originate, spread, and work, “a modest proposal for a start”, he later observed (cited in Levitin, *ibid.*). During his researches he looked at psychological sources – Hoffding, Wundt, Ebbinghaus, Titchener, the Wuerzburg School, and other leading lights of early psychology. As he told his students in 1974, he found these approaches to be so “lifeless”, that they were “responsible for my abiding aversion for psychology, much of which I have preserved to this day” (cited in Levitin *ibid.* 150-1).

On 18 February 1921 the famous “communist-anarchist”, Peter Kropotkin, died in Moscow aged 79. Although he had supported Russia’s entry into World War I and strongly opposed the Bolsheviks, the latter showed great respect for him and his books remained in print until the early 1930s. Luria’s first publication (Luria 1921a), to which he never subsequently referred, was *Kropotkin as a Social Thinker*. It was published in 1921 – presumably as a posthumous appreciation.

Kropotkin advocated the creation of an egalitarian society that would meet everyone’s needs and allow them to develop their potential. He was the most prominent exponent of the view – notably in his book *Mutual Aid* – that co-operation and solidarity within biological species was more common than competition, and thereby offered a better chance of survival. In this he supported Darwin against the so-called “social Darwinists” who promoted the notion of the “survival of the fittest” (cf. Avrich 1988, 57-8). Presumably Luria would have expressed these strong points especially if, as he claims, he was interested in utopian socialism. He says, “The Revolution freed us to discuss new ideas, new philosophies and social systems, especially the younger generation. None of my friends were familiar with Marxism or scientific socialist theory, and neither was I. Our discussions had not got beyond the utopian socialist schemes that were in the air in those days” (Luria 1979a, 18-9).

Luria’s second publication was a preface to and translation of Lujo Brentano’s *Attempt at a Theory of Needs* (Luria 1921b). Brentano (1844-1931) was on the right wing of German Social Democracy – a so-called “Katheder” socialist, or

armchair professor. He wrote on economics and social need (cf. Sheehan 1966). The *Great Soviet Encyclopaedia* accorded him a larger entry than the entry on his brother Franz, the well-known philosopher (written by Vygotsky). In Lujo's entry the noted economist and economic historian I. I. Rubin discussed his writings on wages and social need and also listed Luria's translation in the references (Rubin 1927). Luria's interest in both Kropotkin and Brentano seems, at least circumstantially, to relate to need or, more precisely, need as mediated by society. Although Brentano's title, *Versuch einer Theorie der Beduerfnisse*, has been mistranslated as the "Theory of Human Drives" (cf. Luria 1979, 21), there is no attempt at biological reductionism in it. Nor does Brentano show any awareness of psychoanalysis. Although he does discuss social deprivation, the pamphlet is wide-ranging. At the point when one wonders how he proposes to integrate his many points, he does introduce Gustav Fechner's concept of threshold with respect to stimuli. But his major point, one, which I think, would have been of major interest to Luria, is that, in distinction from other animals, humans require not only physical satisfaction of their needs, but also psychological satisfaction. This latter has parameters set by "the cultural level of the society, the standard of living of the [given] class, and the particular demands of the individual" (Brentano 1908, 65). Human needs are affected not only by cultural and historical development, but also by a need for change, again unique to humans. In a sense, human needs are therefore unlimited. Together with the pamphlet's general discussion, this argument would certainly have opened Luria's mind to non-reductionist approaches to the question of need. Interestingly, although Luria does not discuss Brentano's work in his encyclopaedia entry on "Drives" (Luria and Sapir 1930), it is given in the references, so presumably Luria still recommended it for further reading.

Luria wrote that Brentano's work explored "the needs that set human behaviour in motion and this came very close to the issues that were of interest to me" (cited in E. Luria 1994, 18). This confirms my comment, as does his remark that, together with another work, it "led me to develop a concrete psychological approach to the events of social life" (Luria 1979a, 21). This other work was L.I. Petrazycki's influential 1905 work, *Introduction to the Study of Law and Morality: The*

Foundations of Emotional Psychology. [NB. Petrazycki, a Polish name, is rendered as Petrazhitskii in Russian.]

For us it is difficult to imagine a law book having the cultural impact that this one did. For an alleged law student, such as Luria, it offered an ideal transition from law to the investigation of social and psychological needs. It was cited substantially in Vygotsky's *Psychology of Art* (1971). It almost certainly contributed to the development of the latter's experimental methodology through the "method of provocation" (Petrazycki 1955, 24-5 – cf Vygotsky 1971, 189). Petrazycki is recognized as an influence on social psychology (Petrovsky 1990, 36) and he was a major influence on the Bolshevik commissar for justice and writer on psychoanalysis, M. A. Reusner (N. S. Timarsheff 1955, xxxii). [NB. 'Reusner' is often transliterated as Reisner, occasionally as Reissner, as with his famous daughter Larissa, and also as Reussner].

Petrazycki (1955, 8) wrote that "legal phenomena consist of unique psychic processes... expressed, incidentally, in the unique form of ascribing to different beings, or to certain classes of such beings, 'duties' and 'rights'... ". Yet, he noted, (ibid. 12), that human "capacity to experience the complicated processes that constitute legal phenomena... emerged only with the attainment of a certain level of culture... and in particular the attainment of a certain progress in language." If one is framing laws, one should have some understanding of human psychology and the motivation of human actions. Petrazycki found contemporary psychology unsatisfactory in this respect.

Contemporary psychology, according to Petrazhitskii (ibid. 22-3) was determined to artificially divide human behaviour into three areas:

(1) Cognition (sensations and ideas); (2) feelings (pleasures and sufferings) and (3) will (aspirations, active experiences). [While the first two categories were essentially passive, the third was active. But he sought] experiences in our psychic life not fitting within any one of the three categories, but possessing a bilateral, passive-active nature... These impulse-stimuli – such as experiences of hunger

(appetite), thirst, and sexual excitation – we may term passive-active, bilateral experiences.

He replaced the three category system by (1) bilateral “impulsions” and (2) unilateral experiences – cognitive and feeling (passive) and volitional (active). “In the life of animals and man, ‘impulsions’ act as the principal and directing psychic factors of adaptation to the conditions of life...” (ibid.).

Although the mixed nature of human motivations and actions had been noted before – especially in Pavlov’s analysis of conditional reactions – Petrazycki did not use his ideas to support an overall theory, but spelt out their methodological implications. “The appropriate method of discovering impulsions may be termed that of interaction,” which may be found by the placing of obstacles in the way of normal forms of behaviour. “This experimental method of diagnostics may be termed the method of provocation” (ibid. 24-5). This method was adapted by Vygotsky to understand how young children moved from one “stage of development” to another.

Unfortunately, little of Petrazycki’s work has been translated and I do not believe he extended his theorizing to practical experimentation itself. But the work certainly influenced Luria (cf. Luria 1923a, 23) and his reception of it almost certainly prepared the ground for his own practical experimentation. Petrazycki also raises the notion of the reciprocal recognition of introspection as a “precondition prior to the comprehension of the psychic experience of others. The corresponding scientific methods of cognition may therefore be called the joint method of inward and outward observation” (Petrazycki 1955, 16). Although not an original insight, raising it in terms of the methodology of the psychological investigation of development may well have been useful for both Vygotsky and Luria.

Luria’s concern for methodology led him to read the German neo-Kantian philosophers Dilthey, Windelband and Rickert. “Dilthey was especially interesting because he was concerned with the real motives that energize people and the ideals and principles that guide their lives”. Wilhelm Dilthey (1833-1911)

proposed a *Realpsychologie*, a methodology “in which man would be studied as a unified dynamic system”. It would be “a practical psychology, based on an understanding of people as they live and behave in the world” (Luria 1979a, 22).

Although inspired by Dilthey, Luria criticized his descriptive approach. There is however more to Dilthey – and presumably to what Luria read - than is revealed by the latter’s autobiography. Dilthey’s critique of contemporary psychologies and his methodological conclusions reverberate, admittedly anonymously, in the debates of the 1920s and in Luria’s own experimental research. Dilthey criticized neo-Kantian cognitive theories, positivist, and synthetic or “constructionist” approaches to psychology. His own “philosophy of experience and reality”, which in some respects ultimately derived from the historical approach of Herder, repeated many of the criticisms of contemporary philosophy and psychology that Herder had made (see Appendix I). Dilthey criticized the discrete and partial approach of empiricism and, by implication, associationism, promoting instead the role of the *Wirkungszusammlung*, or dynamic system, both in history and psychology (for the former cf. Makkreel 1992, 314ff).

In *Ideen ueber eine beschreibende und zergliedernde Psychologie* (1894) [Ideas on a descriptive and analytic Psychology], Dilthey wrote, “The law of structure and the character of the mental components is simultaneously teleological and causal” (Dilthey 1976, 92). Without the teleological element - biological development and its role in adjustment to the environment and maturation - there would be no development in life.

The development of a human being cannot be inferred from Schopenhauer’s concept of blind will or from the play of isolated, individual, mental powers described in the systems of [Johann] Herbart or the materialists. Development in man is directed towards securing a pattern of mental life adjusted to his environment. All mental processes co-operate within us to produce such a pattern, giving shape, as it were, to the mind; for even when we distinguish and separate we create relationships and, thus, connections. The formulae of transcendental philosophy about the nature of our synthesizing faculty are only abstract and inappropriate expressions for this quality of our mental life which creates

development and pattern. [He adds], the way these connections are given to us determines the point of view from which we analyse them (ibid.).

Language, myth, religious tradition, custom, law and other organizations are products of the collective mind in which human consciousness, to use Hegel's phrase, has become objectified and so open to analysis. Man does not discover what he is through speculation about himself or through psychological experiments but through history. Dissecting the products of the human mind ... to give us insight into the origin, forms and function of the mental structure must combine the analysis of historical products with the observation and collection of every available part of the *historical processes* by which they were produced. The whole historical study of the origin, forms and workings of men's mental structure depends on the combination of these two methods (ibid. 93).

A clear echo of this can be found in the approach of cultural-historical school that Vygotsky, Luria and their associates founded around 1929. Such ideas clearly had roots in some *psychological* thinking before Vygotsky and Luria had even been born. The roles of biology, history and social experience all have a part to play in the development of an individual.

Dilthey's comments on the methodological implications for psychology are significant in reflecting the practical development of Luria and Vygotsky. "The decisive fact for the study of mental structure is *that the transitions from one state to another, the effect of one on another are part of our inner experience*" (ibid. 94). Luria was quite rightly critical of Dilthey's reliance on mere description of the experience of these transitions. But the study of these transitions themselves was the lifeblood of his and Vygotsky's work in the 1920s.

Dilthey believed we must not simply think of these transitions and states as separate. "Mental life ... is always an encompassing unity. Mental functions have been differentiated in it but they remain tied to their context". With the maturity that development brings "the person is now no longer at the beck and call of stimuli. He inhibits and controls his reactions; he chooses when he will adjust reality to his requirements" (ibid. 94-5). All Dilthey's comments, here admittedly a small selection, undoubtedly reflect nineteenth century thinking – some of it

derived from Hegel. At the same time, if we take them more materialistically, their practical relevance appears, with the hindsight of the careers of Luria and Vygotsky, to be undeniable.

But it is this hindsight that is *crucial* here. Vygotsky's reading of Hegel and the Ukrainian writer on language A. A. Potebnia (1835-91) made him appreciate the role of human development and some of its methodological complexity. The role of language in these qualitative transformations was central to this issue. With Luria it could be argued that Petrazycki and Dilthey raised, at least in part, some of the same issues. It is said that cultural-historical theory arose from the interest of Vygotsky and Luria in the writings of Marx and Engels. It is true that their writings certainly helped Vygotsky and Luria to appreciate and make concrete the issues (cf. Hames 2000). But the seeds had already been planted by other writers. Indeed, as I argue in Appendix I all these issues had already been raised by Herder in the eighteenth century. A large part of what I take to be the misperception of the work of Vygotsky and Luria – especially in the West – is the ignorance and misperception by large sections of the Anglo-American intelligentsia of this whole Herder-derived approach to human development, history and language.

Luria is also presented as being dependent on Vygotsky's depth of learning. While it is certainly true that the older man grasped these issues more firmly and with more assurance, the evidence presented here shows that the 20-year old Luria was in some respects also moving down a similar path. On the other hand, while Luria would have found it difficult to avoid reading the historical, social and developmental elements, his exposure to them does not appear to have led to their absorption into his current thinking. Given that Luria was later considered to have a good grasp of Marxism, it may be that his slowness to catch on to these elements explains his comment that, "Properly speaking, I never really mastered Marxism to the degree I would have liked. I still consider this to have been a major shortcoming to my education" (Luria 1979a, 30). To be fair, Vygotsky, generally considered to have had the advantage over Luria in this respect, did not move into these areas himself in any significant way until the late 1920s. This may be explained partly by the fact that part I of the *German Ideology*, which contains Marx's most significant, though brief, contribution on human nature was not

published until 1924, and Engels' contribution to general scientific theory, *Dialectics of Nature*, the following year – both in Russia.

On the other hand, a practical scientist has to work with the materials available at the stage to which his or her science has developed. It is thus of major significance that, for all their insights, neither Herder, nor any who exhibited his influence, directly or indirectly, namely Marx, Hegel, Potebnia, Pettrazycki and Dilthey, produced a practical psychology. Mere exposure to their ideas was not enough. Confrontation with the practical scientific problems of psychology remained a prerequisite. Here Luria's experience with practical experimentation throughout the 1920s must be considered a major element in the success of both himself and Vygotsky.

II

Engagement with Psychoanalysis and the Schools of Russian Psychology

In 1922 Luria wrote *Principles of Realistic Psychology*, a book “which exists in a single hand-written copy” of 200 manuscript pages. In 1974 he described it as “childish ... but nonetheless interesting” (cited in E. Luria 1994, 19). Using Windelband's distinction between nomothetic and idiographic sciences – those governed by general laws and those concerned with individual processes – Luria suggested that psychology could combine both: studying “the concrete individual man” and revealing regularities, thus making it “a science of individual laws” (ibid.). In 1924/5 he cited his manuscript, *O Printsipakh Real'noi Psikhologii*, as dwelling in some detail on the criticism of the metaphysical use of psychological ‘elements’ (Luria 1994, 173, n.5). He later explicitly states that he was attempting both to use Dilthey's *Realpsychologie* and to place it on a more scientific footing by avoiding Dilthey's descriptive method. The little that he tells us about this work suggests that this was his attempt to resolve Dilthey's shortcomings (Luria 1979a, 23).

Luria's methodological concerns and aversion for existing psychologies is shown by his publication through the student association of *In Search of the Living Man*,

a book by a certain Professor Kruglikov. This work “gave vent to our dissatisfaction with the lifeless, depersonalized, dreary psychology of the time” (cited in Levitin 1982, 151). He also published a book by Konstantin Sotonin called *Temperaments*. He subsequently reviewed this work, and was generally favourable to this “sketch”, but unfavourable to its successor, *The Idea of the Philosophical Clinic*. He criticized the latter for its methodology and failure to use the psychoanalytic approach to psychotherapy. The former appears to have revived the idea of the four temperaments for the sake of creating a symptomatology – as did Pavlov (1928, 390). The evidence of sensitivity to such a temperament, e.g. involving pleasure, was manifest in the speed of reaction to such stimuli. Luria’s reviews (1923c, 102, 104-5) suggest that while he may have found Sotonin’s ‘sketch’ to be stimulating, both works were deeply flawed.

In this Kazan period Luria was groping his way towards a study of human needs, motivations, behaviour and ideas using a variety of sources – economic, legal, ethical, socialist, philosophical, physiological and psychological. Although he writes of his enthusiasm for the Revolution, he offers no evidence of having read any Marxist writings. His focus on and view of the individual reflects the influence of Kant on the authors that he read as opposed to the Herderian influences that appear at times in the work of Petrazycki and, more particularly, Dilthey. Nevertheless, Luria’s interest in concrete, individual behaviour and his frustration with traditional psychological approaches pushed him onwards.

Luria notes that at the time he was wrestling with the problems of Dilthey, he came across the writings of Freud, Adler, Jung and other members of the psychoanalytic school.

Many of Freud’s ideas seemed speculative and somewhat fantastic to me, but the study of emotional conflicts and complexes using the association method seemed promising. Here, I thought, was a scientific approach that combined a strongly deterministic explanation of concrete, individual behaviour with an explanation of the origins of complex human needs in terms of natural science. Perhaps psychoanalysis could serve as the basis for a *scientific Realpsychologie*, one that would overcome the nomothetic-idiographic distinction” (Luria 1979a, 23).

[NB. Whether Luria's manuscript of the *Principles of Realistic Psychology* actually extends to the use of psychoanalysis is not clear (ibid. 24)].

The immensely pro-active Luria, at the age of 20, set up the Kazan Psychoanalytic Society and wrote to Sigmund Freud. Freud responded favourably both to Luria and to the Society's creation. He also authorized the translation of one of his smaller works (E. Luria 1994, 21). It is difficult to follow the precise sequence of both influences and events in Luria's student life. Many of his concerns and interests were developing simultaneously. From 1921 to at least 1925, however, the psychoanalytic element of his work was to be a dominating one. Luria's own contributions to the field are written with both care and caution. He was also to play a major role in the life of the psychoanalytic movement in Russia, and in reporting its activities to the outside world. The Kazan Psychoanalytic Society was organized in the summer of 1922. Of the 14 members, 7 are listed as doctors of medicine, one of the latter was also a psychologist. There were 4 other psychologists, or students of psychology. Luria is listed as "psychologist, president of the Association of Social Science" (Luria 1923b, 397).

When Luria graduated from Kazan University in 1921, his father continued to urge him to go into medicine. Luria remained set on becoming a psychologist, but he compromised and pursued both careers. "At that time it was possible to be enrolled simultaneously in more than one school. So I began taking medical classes and completed two years of medical school before interrupting my studies, which were only resumed after many years. Simultaneously I spent time at the Pedagogical Institute and the Kazan Psychiatric Hospital" (Luria 1979a, 25).

Undoubtedly this increased the range of Luria's contacts at a time when he was building the Society. Dr. R. A. Averbukh was one of the more prominent active members, and another prominent member who joined in October 1922 was Dr. B. D. Fridmann. Averbukh and Fridmann were psychologists. Some of the doctors had also studied in Zurich. They presented clinical reviews of cases of neurosis, and also discussed Freud's theories, sometimes critically (E. Luria 1994, 21). As Secretary of the Society, Luria sent regular reports to the international section of

the psychoanalytic movement. He summarized the talks and reports that were given at the Kazan meetings. His first talk, given at the inaugural meeting on September 7 1922 considered that “experimental ‘mosaic’ psychology, which studied not the personality, but its separate elements, has reached its limits... Psychoanalysis is one ... [new] method of the analysis of personality as a whole, and of late it has established its position as a classical method”. His second talk in October discussed “the psychoanalysis of costume”. Woman dressed in a “sexually passive” way, but men in a “sexually and socially active” way. “The analysis of dress is one method of [obtaining a] psychological symptomatology” (Luria 1923b, 397-8). This was almost the only time Luria spoke of the social aspects of the psychoanalytic concern with sexual behaviour. His daughter adds that the talk discussed times when these rules no longer held – such as during carnival – and also discussed the role of military uniforms and the clothes of those in positions of power and authority (E. Luria 1994, 22ff).

Throughout his active period in the psychoanalytic movement Luria took as full a part in the discussion following the talks as any other member in Kazan or subsequently Moscow. As secretary, he did of course write the reports, but his veracity and efficiency in reporting the talks, and sometimes the discussions, has not been questioned. One discussion, in November 1922, on the relevance of analysis to folklore and literature, agreed that it was relevant, but not always applicable. “The analysis of symptoms –interpretation of dreams, association experiments, and so forth – remains the classical method of psychoanalysis” (Luria 1923b, 398). Given the importance attributed to literature and folklore by some members of the Moscow Psychoanalytic Society, this assessment is evidence of the seriousness with which the members of the Kazan Society viewed their priorities. It also mirrors the concerns of the international movement about the state of the Moscow organization (cf. Miller 1998, 61). Kazan was far from puritan, however; and Luria himself gave a talk on Leonid Andreiev’s play *Savva* four months later (ibid.).

In December Luria (1923d) discussed five distinct directions in contemporary Russian psychology. Firstly, the pre-Wundtian philosophically idealist writers S. Frank and A. N. Vvedenskii (Petrograd) and N. A. Vassiliev (Kazan), secondly,

G. I. Chelpanov (1862-1936) head of the Moscow Institute of Psychology, who urged the importance of experimentation, but at that time still in a largely subjectivist way. He taught many well-known psychologists such as P. P. Blonskii, and K. N. Kornilov, who replaced him in 1923. Thirdly, the Petrograd school of A. F. Lazurskii (1874-1917) promoted experiments that attempted to replicate natural conditions. But by rejecting philosophical and psychological approaches his approach became largely biological. Next Luria addressed the work of I. P. Pavlov (1849-1936) and V. M. Bekhterev (1857-1927). Their competing schools investigated both physiology (including that of the brain) and the forms of behaviour that could be examined by means of physiology. Finally, he mentioned what he called the biochemical trend. Luria's only published comment was that "the original reflexological school of so-called objective psychology represents a special interest for Russia in that, in a number of problems, it comes close to psychoanalysis. [On the other hand,] Russian psychoanalysis is still too young, and has not yet achieved anything original" (Luria 1923d, 115). This conclusion remained central to Luria's thinking throughout his involvement with psychoanalysis.

In his autobiography he explains that while working his way through Jung's *Studies of Diagnostic Associations* and William James's *Varieties of Religious Experience* – both of which impressed him – he came across some papers by Bekhterev and Pavlov.

What immediately impressed me was that both men had objective approaches to problems that psychologists were able to discuss only in subjective terms. I was especially excited by Pavlov's experiments involving conditioning. Most of us have come to accept as commonplace his demonstration that it is possible to measure excitatory and inhibitory processes in the central nervous system which mediate the way in which peripheral stimulation produces salivary reflexes. At the time, however, they were revelatory in their implications (Luria 1979, 25-6).

It was this approach that Luria adapted and which initially helped guide his work within psychoanalysis. Luria's enthusiasm, however, probably relates to the first edition of the work published in the West and now known as *Lectures on*

Conditioned Reflexes volume one as this was only published in 1923 (Pavlov 1928), i.e., a little later.

At the All-Russian Congress of Psychoneurology, held in Moscow on 10-15 January 1923, there were eleven lectures including a repeat by Luria of his talk on clothing and psychoanalysis. Also speaking were well-known stalwarts of the Moscow Psychoanalytic Society. The psychiatrist and historian of psychiatry, I. V. Kannabikh, lectured on psychogenic illness, the analyst and translator of Freud, Dr. Moshe Wulff (1878-1971), on psychaesthesia and phobias, and the writer on literature and editor of the Freud translations, I. D. Ermakov (1875-1942), on childlike play (Luria 1923d, 114). Subsequently when in Moscow Luria showed a recent book of his to Otto (O. Iu.) Schmidt. Together with his wife Vera (V. F.), Schmidt (1891-1956) was another prominent member. Among his many talents, including that of being a famous scientist and (later) a polar explorer, he was Director of the Gosizdat State Publishing House (cf. Graham 1987, 386ff; for references to the whole group cf. Etkind 1997). In 1923 he brought out a new edition of Luria's pamphlet *Psychoanalysis in the Light of the Main Trends in Modern Psychology*. This was a "considerable edition – some five hundred copies" (cited in Levitin 1982, 153).

III

First Major Publication on Psychoanalysis and First Experiments

Luria's 1974 lecture almost gives the impression that this pamphlet was merely an extended series of reviews of books on psychoanalysis originally published in a Kazan literary journal that he subsequently decided might make into a book (ibid. 152). But the comments he makes are far too loose for our purposes. In the preface to the work he writes, "The material for this pamphlet was used for a range of papers delivered at the Kazan Psychoanalytic Society". He states that the survey of psychological and psychoanalytic literature he makes is made for a specific purpose. Namely, "to show the place which psychoanalysis nowadays occupies in the science of the nervous – psychological life of the personality", and "to present a brief survey of the starting points from which the current theories of

psychoanalysis derive” (Luria 1923a, 1). That the work arose from his lectures to the Society is clear from his own reports of the meetings. This is especially evident in his lecture in Kazan on 18 February 1923 on “Some Principles of Psychoanalysis”, which clearly covers some of the same ground as the pamphlet (cf. Luria 1923d, 116). This is also true of another lecture with the same title as the pamphlet, given on 5 March 1923 (Luria 1923e, 238).

There are 46 pages of text in this pamphlet. In the postscript Luria apologizes for the consequences of its brevity, including the omission of such major areas as child development. Seventeen pages represent the final chapter, which comprises two sections. The first is on psychoanalytic “biographies” illustrating the effects of a child’s relationship with its parents. There is an extended commentary on two works on Alexander the Great and Tsar Alexander I. Fortunately there seems to be little mileage in comparing Luria’s character to that of his namesakes. The second section is on collective psychology, together with comments on the role of symbols. This chapter does indeed read like a review of the literature. It also demonstrates Luria’s attitude to history at that time. He concludes that “these works will open the way to an analytical history of religion and the psychology of peoples which will incontestably in future supplant mere historical description” (Luria 1923a, 48). This confirms his comments about his early lack of understanding of Marxism, or indeed of Dilthey’s relation to the historical tradition.

But the earlier, more substantial chapters demonstrate a significant grasp of the overlapping areas of psychoanalysis and psychology. This is particularly evident in the first chapter, “Contemporary Psychology and Psychoanalysis”. Much of this relates to the seemingly perpetual “crisis in psychology”, especially in experimental psychology. In its various discrete studies, conventional psychology failed to concern itself with the functional unity of human behaviour. It failed to study the dynamics either of action, behaviour, personality, psychotherapy or psychopathology. This weakness explained why it was still unable to contribute adequately to applied psychology - such as psychotechnics (the psychology of work), psychotherapy or educational psychology. Earlier psychoanalysis itself is not immune from criticism. “Its weakest link ... was that it was split off from

normal psychology and that it laboured considerably more over the casuistic material of psychopathology than its own principal methodological foundation”(9). Luria’s concern with methodology remains paramount, as before.

But just as Russian psychologists such as Pavlov, Bekhterev, Lazurskii and Kornilov had begun to develop a more dynamic approach, so the faults of psychoanalysis were now being rectified. Implicitly Luria seems to be saying that there is a developing convergence. He hopes this convergence will continue to develop, and this is where his place will be. “Progressive directions in psychological and psychoanalytic thought started to talk the same language and to agree upon similar positions” (ibid.). They shared “the principle of an integral, biological psychology, of the dynamic study and understanding of human behaviour” (10). He found support for this position in statements of Ernst Kretschmer and Eugen Bleuler, and in the positions of several schools including one that later became known as the Gestalt school, Western behaviourism, the “neo-Freudians”, who included W.H.R. Rivers (1864-1922), and the so-called “functional” school, among whom he included M. Ia. Basov. Although these schools were very diverse, Luria was encouraged that their experimental focus seemed to be converging productively. To see the early Luria as an exponent of psychoanalysis, pure and simple, would therefore be very misleading.

The following chapter, “Starting Principles in Psychoanalysis”, made no pretence to be comprehensive, due to what Luria considered the flood of seminal works. Although these theories are widely known, it is useful for us to understand what Luria considered central at that time in order to understand the enormous steps that he took in the 1920s. He discussed Freud’s early examination of apparently trivial phenomena, but even here reactions to stimuli “are regulated by the very same principle which holds significance for the life of the whole personality... Finally, all this mental activity is regulated by the striving towards *pleasure* and by an active repugnance for suffering and pain” (16). In extreme cases of the latter, memories may be forced from the conscious mind.

At the basis of each experience lies the relationship of the personality to the world: attraction or repulsion. Thus, the teleological aspect of all psychological

experiences, fundamental to psychoanalysis, is introduced into psychology. It allows psychology to move from the descriptive to the explanatory... Each psychical phenomenon possesses vital value and significance and is embedded, sometimes unconsciously, in the drives lying at the basis of a given phenomenon. They just have to be laid bare for the phenomenon to be explained... Freud called these principal foundations of the psychoanalytical system [his] 'metapsychology' (18).

In 1933 Freud wrote, "the theory of instincts [drives] is, so to speak, our mythology. Instincts are mythical entities magnificent in their indefiniteness" (Freud 1974, 95). Methodologically Luria had adopted Freud's position and Freud's determinism, but as a scientist, it remained for him to verify these determinisms or, failing that, to exhibit the mechanisms that might be considered consistent both with physiological theory and with such a metapsychology.

His criticisms of traditional psychology's approach to thinking are, however, legitimate. It did assume that thinking was primarily logical and, simultaneously, that it was comprised of abstract, atomized elements. In contrast, psychoanalysis envisaged "consciousness together with its fundamental aspect, thinking, as *functions of the personality as a whole* with its drives, needs, emotions and sets" (Luria 1923a. 19). Luria does not comment here on the apparent contradiction between this position and the views of Dilthey and others on the historical nature of thinking and the possible transformational effects of thinking itself on human behaviour. These were issues that he was subsequently to face during the 1920s, but in stages until he was able to confront, together with Vygotsky, all these issues simultaneously.

In the third chapter, "Teachings of Psychoanalysis on Personality and its Drives", Luria admits that tracking down the drives beneath the observable psychical phenomena is a major task, indeed their study is "the most fundamental and difficult issue facing psychoanalysis" (21). He also notes that in the work, *Drives and their Vicissitudes*, Freud defines a drive as an internal stimulus, which manifests itself in many ways (cf. Freud 1984b, 114ff). Even the sex drive, the prototypical drive, may be manifested basically, "crudely", or through the

mediation of many forms of social 'refinement' or variation. Later Luria discusses the ego's instincts concerned with the needs of self-preservation and self-assertion, and Alfred Adler's corollary to it, the inferiority complex and forms of compensation.

The mechanisms of the operation of drives followed the reflex model, as Freud pointed out in *The Interpretation of Dreams* and subsequently (cf. Freud 1976, 686, 719ff, 757). Although the work of Pavlov and Bekhterev in this field was important to Luria, he considered of special relevance those places where psychoanalysis independently discovered, by its own methods, mechanisms that reflexology had also found. These included not only the basic stimulus-reaction or inhibition, but the repression and revival of unconscious "traces", together with displacement as the basic mechanism of neuroses. As we shall see, this concentration on finding the connections between parts of the dynamic nervous system was to prove a major interest to Luria throughout his life. Although he later rejected Freud's metapsychology, acquaintances of Luria of a psychoanalytic persuasion believed that throughout his life he utilized approaches associated with psychoanalysis. Writers on psychoanalytic movements often tend to believe 'once a psychoanalyst, always a psychoanalyst,' even if covertly. Although Luria was extremely enthusiastic about psychoanalysis in his youth, he was above all a scientist. If he did remain open to Freud and some of his concepts, it was because he, as a scientist, found them to be useful and consistent with his own ideas. But it was merely one source of ideas, and a declining one at that, among many others.

Luria refers also to Freud's latest work, *Beyond the Pleasure Principle* (1920). Here repetition, Luria says, is for Freud, "the *fundamental mechanism, the basic principle according to which the entire psychological life is constructed*" (Luria 1923a, 31-2). At this stage Luria does not elaborate on the many forms repetition takes in this work, notably Freud's speculations on biological regression.

While still at the Kazan Psychiatric Hospital Luria undertook his first exploratory studies of psychiatric patients. These patients included Dostoevsky's granddaughter. "While I was able to fill notebooks with her free associations, I was in no position to carry out my plan to use such data to capture 'the concrete

reality of the flow of ideas'. In fact, just posing the problem in this way makes it clear why such an approach could lead nowhere" (Luria 1979a, 24). This may be taken to mean that he found the existing form of free association to be unsatisfactory in terms of understanding the dynamics of the disturbed mind, and possibly of locating the patient's hidden concerns. Obviously, from a therapist's point of view, such an approach is valuable only in certain situations. For Luria, attempting both to extend and ground psychoanalysis scientifically, this approach failed not only to locate the sources of the complexes, but the structure and dynamics of the disturbances. Luria does not appear to have attempted other forms of psychoanalytic therapy. It may explain why in the postscript to his pamphlet Luria wrote that he "did not touch at all on the psychoanalytic teachings on neuroses, an area with which this author feels incompetent to deal" (Luria 1923a, 49). But, in this context, we can appreciate that, for Luria, the limitations were not restricted to those of his own personal abilities. He was to return to the subject later.

Luria was more successful with his first experiments. He took a job as a laboratory assistant at the Institute of the Scientific Organization of Labour. His experiments took place at a printing works where he was also employed. One concerned the effects of fatigue of workers in the type foundry where heavy metal intoxication was a major hazard. The other experiment involved a reflexological method of studying suggestibility (Luria in Levitin 1982, 153).

The former experiment studied the effect of hard work on mental activity. He used an old Hipp chronoscope he had found "to measure the influence of verbal instructions on [the workers'] reaction time. It was my first attempt to discover the role of speech in regulating reaction time" (Luria 1979a, 26). He was not impressed by the results though.

The other experiment involved the use of a Kornilov dynamometer, which he and his colleagues had obtained.

[This machine] was just a convoluted pipe with mercury. By pressing a valve on top of a cylinder one could measure not only the speed, but also the intensity of the

reaction. A curious fact attracted my attention. The dynamometer curve was recorded on a smutty drum and was usually regular in shape, but was irregular when the person tested revealed an affective reaction, i.e., when he was for some reason concerned about the experiment. That observation would later play an important role in my life...(cited in Levitin 1982, 153)

The subject's speed of reaction was measured, and then Luria would suggest to him that he was growing weary. In the next round of tests Luria "found that the reaction time became much longer. I thought that the increase during the intervening period could be used as a measure of the individual's suggestibility" (ibid.). The title of Luria's paper, *Toward a Method of Psychological Investigation* (Luria 1922), clearly suggests his awareness of its potential methodological significance.

Luria's attempts to get his results published demonstrate considerable enterprise. First he decided to publish a journal, then persuaded Professor N. A. Mislavskii, a noted physiologist, to be an editor. (Luria's father had studied for his doctorate at Mislavskii's physiological laboratory). Next he made his first visit to Petrograd to invite no less a scientist than the eminent V. M. Bekhterev to be an editor. Luria hinted that he would be available as secretary for the journal. Bekhterev agreed providing "that the word 'reflexology' be added to the title I suggested for the journal, *Problems of the Psychophysiology of Work Hygiene*. It was duly added. Finally Luria went to Krestovnikov's soap factory and procured some yellow wrapping paper on which to print the journal (E. Luria 1994, 25, cf. Luria 1923f).

According to Luria's autobiography these articles were seen by Professor K. N. Kornilov (1879-1957), newly appointed head of the Moscow Institute of Psychology, and attracted his attention. According to Luria's 1974 talk:

I wrote a letter to Kornilov in Moscow in which I told him that I had read his book on reactions, found it interesting, and was enclosing my own papers on the same subject. To my great surprise [!], I received a letter from him with an invitation to

come to Moscow ... Kornilov was casting about for pupils, and here was a young provincial lad also working with a [Kornilov!] dynamometer. Why not invite him? In the autumn of 1923 I joined the staff at the Institute of Psychology... Incredible though it may sound, I was appointed academic secretary (ibid. 154).

There appears to have been another pressing reason for Luria to move to Moscow, which would also suggest that he was actively seeking the move. This was to assist in the reorganization of the Russian Psychoanalytic Society. In 1922-3 Russia contained one eighth of the membership of the International Psychoanalytic Association. Kazan's membership was slightly more than Moscow's. Kazan had already joined the Association. The Moscow group's application was blocked by those members of the international movement, who believed that there should be only one nationally centralized association. The negotiations for Moscow's entry were protracted and complicated. This situation was resolved by combining the Kazan and Moscow societies into the All-Russian Psychoanalytic Association, which was definitively admitted to the International Association in April 1924.

Apparently Luria was charged with facilitating this change. On 4 September 1923 the Kazan group decided that "in view of the centralization of the Russian psychoanalytic movement, it is considered desirable for members of the Kazan Psychoanalytic Association to enter the All-Russian Psychoanalytic Association based in Moscow. For the moment it has been agreed to transfer A.R. Luria, and doctors B. D. Fridmann and R. A. Averbukh to Moscow" (cited in Marti 1973, 219-221). This was probably to raise the proportion of medically trained members in the city and set a more serious medical orientation in the organization (Miller 1998, 61). This resolution appears to have been simultaneously passed in Moscow. Luria became secretary of the reorganized Association. He was barely twenty-one.

Luria's own assessment of this period is important and, I feel, both correct and worth repeating. "Throughout this period of my life I was naively groping. Still, after fifty years, I have the feeling that many of these activities were significant in my further development as a psychologist. In later years the surface appearance of

my research changed a great deal. But the central themes that had guided my initial efforts remained” (Luria 1979a, 27).

Chapter 2: Moscow: The First Years

I

The Psychoanalytic Society, the Institute of Psychology, and ‘the Combined Motor and Verbal Response Method’

Another possible reason for the move to Moscow was Luria’s forthcoming marriage to Vera Nikolaevna Blagovidova, also a student at Kazan University. She became an actress at the Moscow studio of Alexander Tairov, one of post-revolutionary theatre’s ‘big five’. In Moscow Luria’s interest in painting, poetry, architecture and theatre developed, and he became friends with Sergei Eisenstein. Luria never referred to his first wife in his autobiographical writings. She fell in love with another man, perhaps when Luria was in America in 1929. Luria accepted this and they remained on friendly terms, but lost touch. After her parents’ deaths Luria’s daughter (by his second wife) found Vera and wrote about her (cf. E. Luria 1994). After their divorce Luria moved in briefly with his parents, who had recently moved to Moscow.

Over the course of the next three years Luria continued working in the overlapping areas of psychoanalysis and psychology. The period was marked by his efforts to reconcile the two, and contains his major psychoanalytic writings. He also began the long series of experiments that was to lead to his *doktorat*. The arrival of Vygotsky in Moscow in 1924 made a substantial impact, and this busy period also marks the beginning of their collaboration. The 1920s as a whole may be viewed as a long period of fermentation that led to the major theoretical advances of the late 1920s, but there were also notable events and decisions made, not simply in Russia at large, but in the theoretical development of both Luria and Vygotsky.

In 1924 Luria reported that in the autumn of 1923 several new members had joined the Russian Psychoanalytical Society in Moscow, including Averbukh,

Fridmann and himself from Kazan, and Sabina Spielrein (1885-1942), the well-known analyst recently returned after many years in Western Europe. Luria described himself as currently assistant at the Institute of Psychology. The first meeting of the Society took place on 18 October 1923, so we may assume that Luria had already settled in Moscow by then.

As secretary of the Society, Luria reported on both it and the State Psychoanalytical Institute with which it was closely associated. A committee conducted the business of both Society and Institute. This comprised the president, vice-president, secretary and two committee members, respectively: Ermakov, O. Iu. Schmidt, Luria, Spielrein and Wulff. (Schmidt, Spielrein and Wulff preferred the transliteration of their names back into their original German as opposed to the conventional Shmidt, Shpilrein and Vulff). The Institute was founded in Moscow in 1921. Until the autumn of 1923 its primary role was as a children's home and laboratory on psychoanalytic lines, but in the autumn of 1923 its work was greatly extended (Luria 1924a, 258).

Thanks in particular to Alexander Etkind's findings we now know a great deal more about the Institute. There was a close connection between leaders on the left of the Revolution and the Institute. In his published writings Luria never touched on this, but in the manuscript of the 1974 lecture he mentions that "Radek and a slew of others" supported it (cited in Etkind 1997, 197, though not in Levitin 1982). Karl Radek, second husband of Larissa Reusner, and son-in-law of M. A. Reusner, was a leading supporter of Leon Trotsky and the Left Opposition to Stalin and his associates in the mid-1920s. It seems to be unarguable that Trotsky and his colleagues played a key role in obtaining state support and financing for the Institute from 1923-5 (Etkind 1997, chapter 6). Viktor Kopp, as ambassador to Germany and a member of the Society (*ibid.* 250-1), probably solicited and obtained financial support from Germany, including food shipments for the children's home, known as the International Solidarity Children's Home. In addition to such luminaries of the Revolution, Luria probably met other members of the government. It is thought that several left their children at the home, when they were abroad or away from Moscow on government business. Luria later informed M.G. Iaroshevskii that Stalin sent his son there (Iaroshevskii 1994, 36).

Luria reminisced about the luxurious Riabushinskii mansion, where Gorky subsequently lived, that housed the Institute. “I had a splendid office with silk-lined walls in which I sat with an air of great solemnity and which was the scene of fortnightly psychoanalytic meetings. The first floor was occupied by our psychoanalytic society and the second floor by the psychoanalytic kindergarten”, i.e., the children’s home (cited in Levitin 1982, 160).

At the Institute of Psychology “the staff were young and inexperienced. None was older than twenty-four, [apart from Kornilov, the director,] and few had proper training, but everyone was extremely enthusiastic...” (Luria 1979a, 31). Kornilov sought to distinguish his school of “Reactology” and the work of the Institute from that of Pavlov and Bekhterev. By attempting the objective measurement of mental behaviour through reactology, he proposed to lay the basis of an objective and materialist psychology, which would overcome psychology’s separation from or reduction to physiology. In 1921 he wrote, “what we label psychical processes are little more than a particular kind of physical energy” (cited in Rahmani 1973, 25). The institute was supposed to reform the whole of psychological science by abandoning Chelpanov’s idealistic approach. Kornilov spoke in 1923 of the need to apply Marxist philosophy to psychology” (ibid.). Luria (1979a, 31) notes that the study of reactions involved a wide range of work including rats and mazes, the motor reactions of adults, and problems of education. Naturally Kornilov’s dynamometer was to play a major role in the study of motor movement. As regards the teaching programme Luria followed the experience of many other new teachers by keeping a day ahead of his students. L.V. Zankov and I. M. Soloviev were among his students.

Luria wrote (ibid. 31-2), “It is difficult to characterize my feelings at the start of my professional life except perhaps to say that they were highly ambivalent. I was in full sympathy with the Institute’s efforts to develop objective methods of research. I did not think much of the efforts to measure mental energy; Kornilov’s mechanistic scheme was clearly an oversimplification.” But he *did* regard it as a step forward. Kornilov published the work of his staff including some of Luria’s subsequent work, and seems to have considered it significant (cf. Kornilov 1930, 277). Nevertheless Luria reported that “differences with Kornilov began almost

from the beginning as we did not like his approach” (E. Luria 1994, 32, also cited in Levitin 1982, 155). Although Luria had theoretical and methodological differences with Kornilov, professionally he seems to have had a fairly free hand. He was put in charge of his own laboratory, titled the “Affective Reactions Laboratory”. He was also provided with staff for this, including a younger colleague, who was to work with him for much of the 1920s, and in later life. This lifelong friend, Alexei (A. N.) Leont’ev (1903-79) was also to prove as adept as Luria in improving his experimental apparatus (*ibid.* 156).

Over a short space of time, the Institute developed, and its departments were headed by major figures in Soviet psychology. Luria reported that his own department, that of General Experimental Psychology, was led by Kornilov whose co-workers were Pavel Blonskii (1884-1941), the educational psychologist, and N. A. Bernshtein (1896-1966), now recognized as one of the world’s leading scientists in the physiology of movement. Professor M. A. Reusner headed the Social Psychology department, Isaak Shpilrein (1891-1937), brother of Sabina Spielrein, headed Applied Psychology, A. B. Zalkind (1888-1936), Psychopathology, Nikolai Rybnikov, Child Psychology, and V. M. Borovskii, Animal Behaviour. In his department, Luria was “scientific co-worker of the first rank”. His role in this key department was to prove pivotal for the experimental investigations of Vygotsky and his associates. (For further details of Luria’s 1926 report see Van der Veer & Valsiner 1991, 128-31).

Two of Vygotsky’s biographers make a plausible case that the methodological framework that Kornilov provided gave scope for Vygotsky to develop when he joined the Institute in 1924 (Van der Veer & Valsiner 1991, chapter 6). As a corollary of this they argue that Luria probably did not hold critical views of Kornilov at the time, and from my comments this also seems plausible. On the other hand they do accept Vygotsky’s critique of Kornilov from 1926. He wrote that Kornilov’s “new system lays the concept of reaction – as distinct from the reflex and the mental phenomenon – at the basis of a third way in psychology. The integral act of the reaction includes both the subjective and the objective aspect”. That is, “the new theory accepts the doctrine of psychophysical parallelism”, and so forth (Vygotsky 1997b, 314). On the other hand “the works of Kornilov are the

beginning of this methodology [to resolve the crisis in psychology], and anyone who wants to develop the idea of psychology and Marxism further will be forced to repeat him and continue his road. As a road it is unequalled in strength in European psychology. [It could]... lead to the creation of a theory of psychological materialism" (ibid. 332). This was because Kornilov's 'reactology' advanced beyond Pavlov by admitting consciousness as a legitimate subject of study for psychology. Reactions could be studied in more fields than Pavlov was then prepared to admit. As Yaroshevskii puts it, "It was due to this theory that a decisive impetus was given to the movement of Soviet psychology towards Marxism" (Yaroshevsky 1989, 136).

The fact that Luria reported his criticisms of Kornilov both in print and in the 1974 talk suggests to me that he was serious. Since, like Vygotsky, he also both praised the step forward Kornilov had made, it suggests that they were of like mind on this. To suggest that Luria's recollections are "armed with hindsight concerning Kornilov's 'fall into disrepute' in the early 1930s" (Van der Veer & Valsiner 1991, 127) is unworthy, especially as Luria does not repeat the criticisms that accompanied this fall. That Vygotsky's biographers do not impute the same motives to Vygotsky leads one to speculate on their impartiality.

Luria decided "to try and create an objective psychoanalysis, i.e., to devise a way to measure affective experiences and complexes in some objective way, for example, in reactions" (cited in Levitin 1982, 155). He also describes this approach as "experimental psychoanalysis" (1979, 32). Strictly speaking, this involved experimental situations that he saw at that time as both relevant and significant for psychoanalytic theory and practice, but were not exclusively confined within that framework. Indeed when he wrote *The Nature of Human Conflicts* (1932a), based on his researches, he had moved beyond psychoanalysis and was still able to locate these experiments within his new framework.

Luria describes the development of his famous combined motor and verbal response method, a term usually and misleadingly abbreviated to the "combined motor method". He describes how he found Kornilov's dynamometer to be too crude for his purposes and replaced it with an approach based on Ermakov's

apparatus. This was “a pneumatic table with an aluminium plate attached. It was used to study the dynamic components of writing: a person wrote on the plate and a pneumatic receiver reflected the pressure, all of which was then recorded on a drum. It was a far more sensitive instrument, showing the character and form of reaction, the writer’s degree of confidence...” (cited in Levitin 1982, 155). The change introduced by Luria was substantial and its object different. Its aim was not simply the recording and timing of the verbal responses, but also the measurement of the motor responses that accompanied the verbal responses. This was achieved by replacing Ermakov’s pneumatic table with two pieces of equipment, each containing a rubber ball (or bulb), one for each hand. The subject was to press the ball with his right hand simultaneously with his response to the given word, while the left hand was to remain passive. In instances where the experimenter’s word provoked an emotional response in the subject, delays in responding, together with tremors manifested in the recording of the movements of both hands, were evident. The active hand, usually the right, was assumed to convey the effects of the central nervous system. The passive hand was to monitor the effects on the peripheral nervous system, especially on occasions when disturbances spilled over into that area. [Photographs of the apparatus can be found in Luria 1932a, 25-6, and E. Luria 1994, the eighth photo following page 96]. In some experiments he “also measured breathing, pulse rate and electro-physiological changes” (Luria in Cole & Cole 1971, 79; cf. also Luria 1932a, 27).

It was a major development of word association experiments. As we know, Luria was familiar with Jung’s *Studies of Diagnostic Associations* (1907). Comments by Jung from this work and associated papers are quite revealing. “Galton, Kraepelin, Aschaffenburg, Sommer and others have introduced into psychology a very simple experiment in which a word is called out to the subject, who must respond as quickly as possible with the first word that occurs to him. The reaction time between the stimulus and the response can be measured...” (Jung 1973, 524). Jung accepted that these reaction times demonstrated affective phenomena (ibid. 546). From Jung’s account it is clear that such experiments were far from being the exclusive concern, let alone the invention of psychoanalysis. In one study he refers to its use in criminal cases, and cites experiments by William Stern, Hans Groos, Max Wertheimer and Julius Klein (ibid. 318, 328-9). But naturally the

method was attractive to psychoanalysts. “No one can get out of his own skin. We act as our psychological past, i.e., as our cerebral organization dictates. For this reason we have to expose ourselves in the association experiment in exactly the same way as we do in our handwriting” (ibid. 420). Jung found that “by means of the association experiment, aided by Freud’s psychoanalytic method, I have succeeded in proving that all neuroses contain certain complexes, whose disturbing influences have a disease-producing effect”. But he also accepted “that more or less autonomous complexes occur everywhere, even in so-called normals” (ibid. 602). Thus even in psychoanalytic writings, the association experiment was not necessarily to be interpreted in terms associated exclusively with psychoanalysis.

As we saw in the previous chapter, Luria felt that psychoanalysis had much to learn from Russian physiology and reflexology. In many respects they appeared to speak the same language. In *The Interpretation of Dreams* Freud wrote, “All our psychical activity starts from stimuli (whether internal or external) and ends in innervations”. As regards the psychical apparatus: “its first structure followed the plan of a reflex apparatus, so that any sensory excitation impinging on it could be promptly discharged along a motor path” (Freud 1976, 686, 719). Similarly Bekhterev, founder of the school of reflexology, wrote before his death in 1927, “It is true that [Freud] intends this comparison to be taken figuratively but, clearly, in discussing complex psychic phenomena, even he cannot dispense with the scheme of the reflex”. Indeed “we cannot help seeing the correlation of reflexology with Freud’s doctrine... first of all in so-called catharsis... which is equivalent to discharge of a ‘strangled’ affect...” (Bekhterev 1932, 417, 413). None of this should be surprising since the ‘father’ of Russian physiology, I. M. Sechenov (1829-1905), had written in 1866, “All psychical acts ... develop by way of reflex. Hence all conscious movements resulting from these acts and usually described as voluntary, are reflex movements in the strict sense of the term”. “There are [however] many [sequences of] psychological reflexes whose last member, i.e., movement, is inhibited” (Sechenov 1965, 80, 89; cf. Todes 1981).

Both Pavlov and Bekhterev were far from uncritical of Freud, but since all saw energy flow as the basis of their systems, each had their own versions of inhibition, displacement, regression, neurosis, and so forth – each using similar or identical terminology. Thus it was to be expected that many Russians, both amateurs and professionals, would see these schools as compatible. M.G. Iaroshevskii quotes an American visitor reporting Pavlov as saying that reading Freud helped him arrive at the concept of inhibition (cited in Etkind 1997, 239). In view of Sechenov's comments above we may consider that, if true, Pavlov was being excessively diplomatic. By comparison, it is usual in the West to see psychoanalysis as relatively self-sufficient.

In 1923 Trotsky wrote, "it is clear to anyone, even the uninitiated, that the work of our physiologist Pavlov is entirely along materialist lines. But what is one to say about the psychoanalytical theory of Freud? Can it be reconciled with materialism as, for instance, Karl Radek thinks, and I also, or is it hostile to it" (Trotsky 1991, 247)? In exile in Vienna before the First World War, Trotsky and Kopp came to know psychoanalysis through their friend Adolf Ioffe, and actually attended meetings. Ioffe, a leading Bolshevik both before and after the Revolution was involved with psychoanalysis both as a patient and a writer. He apparently wrote for the psychoanalytic journal *Imago* (as Ioffe), as did Sabina Spielrein and Vera Schmidt (cf. also Miller 1998, 186, n.40). He does not appear in the minutes of the Psychoanalytic Society as he spent much of the time as an ambassador. Although Trotsky famously said that the Revolution did more for Ioffe's sanity than analysis, Etkind (1997, 234) states that in 1924 when ambassador to Vienna, he was accompanied by his psychiatrist, Kannabikh. In a letter to Pavlov in 1923 Trotsky acknowledged that as regards psychology he was something of a dilettante. He seems to have been familiar with Freud's work *Beyond the Pleasure Principle* (1920), for he says to Pavlov. "Your studies of conditional reflexes often seem to involve the theories of Freud". In the latter "psychological processes are presented as a complex superstructure", but Freudians themselves "look into a deep and relatively dark well", and "even make a series of witty and interesting, yet scientifically arbitrary guesses about the attributes of the bottom [of the well]" (Trotsky 1927). Here Trotsky's metaphor echoes Pavlov's own 1910 comparison of physiologists building the base and psychologists the superstructure (Pavlov

1928, 113). Could Pavlov provide some scientific anchor for these “half-scientific, half-belletristic methods” (Trotsky 1927)? Trotsky persevered with his hopes for psychoanalysis until his death, as well as his criticisms of it (see Hames 1999, 49, n. 5 for the references).

In view of the wealth of contemporary testimony to the significance and relevance of psychoanalysis to psychology and physiology, it would be a mistake to treat Luria’s involvement with the former simply as a naïve youthful association as he later portrayed it. Luria was not ready to confront the more dramatic elements of Freud’s metapsychology. But from the start Luria seems to have followed Bekhterev’s view that Freud’s earlier position was mistaken in attributing the psychoneuroses exclusively to sexual trauma, and recognized that Freud’s subjective approach needed objective experimentation to validate it.

Vygotsky also considered the relevance of psychoanalysis for speech and thinking. In 1925 he considered that the development of methods to demonstrate the reactions between them was most important. “Psychoanalysis is one of these methods” (Vygotsky 1997c, 74). Indeed “the unconscious mind also refers to reflexes that have not been translated into other systems”. It is a corollary of the view that “to be conscious of one’s experiences means nothing less than to possess them in object form as a stimulus for other experiences”, and that consciousness provided “the mechanism for translating reflexes from one system into another” (ibid. 71). Perhaps here he was commenting obliquely on the work of his new colleague, Luria, to which we will now turn.

II

On the Historical Significance of Luria’s Method, and its Application in the 1924 Moscow University ‘Examination’ Experiments

Luria and Leont’ev published their report on their first joint experiment two years after it took place, (Luria and Leont’ev 1926), though the first translated version appeared in 1932 (Luria 1932a, chapter II, section A). Their rationalization of its organization might initially appear straightforward and empirical. In the 1970s Luria said “we assumed that emotional disturbances led to the disorganization of

behaviour and to neuroses. The problem was to demonstrate these phenomena objectively” in terms of the symptoms and mechanisms, and indeed the therapeutic implications. [But, in reality], “our stimulus - the word – evokes a very complicated, hidden, neurodynamic process. Sometimes the process of generating a speech response is regular, at other times there is emotional vacillation or obstruction of the process. Every sharp fluctuation in bulb pressing, every tendency to delay the speech response, suggests emotional disorganization, and both the associative process and the motor reaction are changed” (Luria in Cole & Cole 1972, 79).

In hindsight the key question begins to emerge – what is this “very complicated, hidden, neurodynamic process”? Freud’s speculations and the investigations of Pavlov had opened a vast field. Kornilov had sought ways of measuring mental energy. Jung had adapted word association tests for new investigatory purposes. Luria had come up with an experimental procedure that began to objectively measure and show the systematic nature of all these areas of investigation. As Radzikhovskii and Khomskaia put it (1981, 8), Luria’s discovery of the combined verbal and motor method “had one latent aspect”, it was “a kind of ‘methodological time bomb’”. At the time “the main difficulty was how to interpret the results obtained [in terms of *systems*-MH]. It was impossible to build up any integral notion about the sphere of human emotions and motivations (although this was, in the final analysis, where Luria was heading), without a definite general psychological theory. But a psychological theory capable of assimilating the data obtained by Luria simply did not exist [at that time]” (ibid. 9).

The above comments are probably the most profound to have been made about Luria at this stage of his career. Because of his self-effacement it is easy to fall into a view of him as working through a series of experiments, but otherwise seemingly not being able to capitalize on them in terms of major theoretical advances. This could not be further from the truth. The gap between the results and a theoretical framework was simply too wide to be bridged at that time even by the collaboration with Vygotsky. But the impression that Luria was rescued from this dead end by being diverted by Vygotsky into other lines of work in the

late 1920s is over-simplistic in the extreme. In reality Luria (and Vygotsky) spent the 1920s examining various theories and approaches with a view to developing and amplifying all their work by evolving their own theoretical framework. Hence all the later work was related in one way or another to the implications of this work, which may be expressed by the question ‘how do the higher mental processes operate from a systemic point of view?’ The later work can appear to be separate, but in reality the two sets of work are intimately connected.

Radzikhovskii and Khomskaia (ibid. 19, n.1) raise the opposite question to the one normally raised, “What role did this contact with Luria have in Vygotsky’s scientific career?” Their article does not attempt to answer it. The methodological time bomb they mention is seen by them as “important for understanding Luria’s subsequent scientific career” (ibid. 8), but the questions it raised surely also stimulated Vygotsky’s thinking. Luria’s assessment of this relationship tends to confirm my own.

My own work was permanently changed by my association with Vygotsky and the ingenious studies of our students. At the same time that we were carrying out this new line of work [into child development], I was still conducting studies using the combined motor method ... but the focus on my work began to change. Although I had begun with an interest in studying the dynamic course of emotions, *Vygotsky saw in my research a model for studying the relation between complex voluntary movements and speech. In particular he emphasized the way in which speech served as an instrument for organizing behaviour* [my emphasis - MH] (Luria 1979a, 51-2).

In the combined verbal and motor procedure lay great possibilities for studying the processes engendering affect. Although, as we have seen, Luria did admit to measuring breathing, he regularly pointed to the limitations of using pulse, blood pressure and breathing as Wundt and others had done (e.g. Luria 1932a, 170-1). His method, in contrast, could both measure in which direction a process was progressing, and which processes were involved. In a paper written in 1928 with M. S. Lebedinskii on using the method to assist in diagnosing functional neuroses and other nervous disorders and diseases he elaborated further.

Repeated experiments demonstrate to us that in the representation of a motor curve, a whole range of interesting and, for the illness concerned, characteristic processes find expression. For this very reason the associative reaction and the motor impression in our experiments form, as it were, a *unified system of functions* [*einheitliches Funktionssystem*]. Every change in the central processes finds its expression in the motor indicator: every disability, fatigue, inhibition, as well as affective agitation alter in intensity the discharge of the motor reactions (Lebedinsky & Luria 1929, 474).

In this we can see not only the connection and contradiction of different processes, but begin to understand the nature of the operation of the brain. It operates not simply in discrete localized units or modules, nor as an overall form of activity – but in terms of specific changing neural pathways and functions - or, as they came to be called ‘functional systems’. This is what I understand by the application of the term ‘time bomb’ to Luria’s method. Luria’s concept is, of course, distinct from Anokhin and Bernshtein’s self-regulating system. In contrast, the stimuli, that may initiate an activity, which may be linguistic and conditioned by cultural practices, are part of the system as well. The German-American linguist Edward Sapir used the term in respect to language. “If language can be said to be definitely ‘localized’ in the brain, it is only in that general and rather useless sense in which all aspects of consciousness can be said to be ‘in the brain.’ Hence we have no recourse but to accept language as a fully formed functional system within man’s psychic or ‘spiritual’ constitution. We cannot define it as an entity in psycho-physical terms alone, however much the psycho-physical basis is essential to its functioning in the individual” (E. Sapir 1921, 9). Sapir’s book was published in Russia in 1933 or 1934, and used by Vygotsky in *Thinking and Speech*. There is no evidence of Vygotsky or Luria being aware of this book before then. Luria appears to have used and then *developed* the term independently, and *if* Lebedinskii and Luria’s use of the term ‘system of functions’ is its first appearance in this sense, then it can be attributed to the implications of using Luria’s ‘time bomb’. Needless to say, the implications of this for Luria and Vygotsky were enormous. I have felt the need to offer this ‘advance warning’ of

the implications of Luria's early work, but this is not the appropriate chapter to elaborate it further.

A comparison of the theoretical issues, which Luria raises in the introduction to his account of the following experiment, with the design and reports of the experiment itself, demonstrates total consistency of viewpoint. This suggests that the published accounts of the experiment perhaps altered but little between 1926 and 1929, the year of the latter's arrival at the publishers. Khomskaia's description of Luria's approach to the organization of experiments is instructive here. "He always planned his activity according to the demands of a particular goal and tried to obtain a concrete result for his work. 'The image of the result', as in Anokhin's approach, was always present in his mind..." (Homskaya 2001, 115). If we accept that the theoretical implications of both the theory and the report of the experiment today seem so wide reaching, then it is impossible to believe otherwise than that some of the potential was evident *at the time*. It is true that there is evidence of the use of terminology used by the Gestalt-influenced neurologist Kurt Goldstein, (*Nahe- und Fern-wirkung, Kurzschluss*) but this has no apparent bearing on the experimental design itself, and may indeed already appear in Luria's first Russian account of the experiment (Luria & Leont'ev 1926). As we shall see in Chapter 5, Luria's thinking was already showing the impact of the Gestalt school in 1926.

Luria begins the introduction, "The Problem of Neurodynamic Investigation of Affect", by pointing out that usually the symptomatology of affect was portrayed simply as the systemic disorganization of active human behaviour. How the separate connecting systems related to each other within this process was insufficiently studied. Luria admitted that his aim was not to describe all the symptoms himself, but he did feel that it was possible to differentiate cases of generalized diffuse affect from those where a "conspicuous contour of the concentrated affect, which appeared only in association with definite stimuli. [These stimuli] extended only to certain reactive systems that manifested a very definite structure, whose forms we were able to study" (Luria 1932a, 44). Although properly speaking, a structural state was not a very satisfactory term, "even the affective chaos ... always manifests a lawfulness". Included within this

dynamic there usually appeared a stage of diffused affect, but even so this sequence and its recognizable stages needed examination.

As regards the mechanics of affective states few, save Pavlov and W. B. Cannon, had undertaken any physiological research. Did “the affect change the very nature of the reactive process?” In terms of the dynamics of the whole process, however, its “course ... only becomes comprehensible when we take into account the leading role played by the higher forms of behaviour, and the more complex psychological systems”(45). But what other systems did it change – did it extend beyond the active, voluntary systems? He concludes, “our investigation proceeds from the limits of the neurodynamic analysis and becomes significantly wider, truly psychophysiological” (46).

The traumatic effects of examinations and their consequences are now familiar to millions of people. Actually the ‘examination’ Luria refers to in his study of examination trauma was a review by a commission of the student’s work. It arose because, since the Revolution, access to universities had been open and free, but shortages of staff and facilities prompted the decision to undertake a *chistka* – a purge or ‘weeding out’ of those in the university who were deemed to be academically deficient. Of course the idea of a purge may remind the reader of Stalin’s purges of the 1930s, but since Lenin was dead but a few months and Stalin was not yet exercising power one assumes that this was basically the sort of weeding out that occurs in western universities. There was however an element of positive discrimination in that those deemed both academically deficient and socially privileged led the exit. Unfortunately it is difficult to judge this aspect on the information provided by Luria (ibid. 47-8). I may prove to be wrong on this, but as someone whose own teacher’s reported responses certainly suggested that I was personally subject to negative class discrimination regarding my applications for Oxbridge in the 1960s, I find the case for positive discrimination more appealing. In the Russia of the early 1920s – a land of mass deprivation and illiteracy - it would seem elitist to dispute it.

Nevertheless Luria was well aware that this experience would be more traumatic than other forms of ‘examination’. “This had very specific conditions for the

investigation of an acute mass effect”. Although he notes previous such experiments, his own went further in that “we took students directly from the line awaiting ‘examination’, so that some of them were ‘examined’ only a few minutes after our experiment” (48). Here one can sympathize with his modern-day critics. This would certainly be prevented today by a committee on the ethics of experimentation. Luria’s case would have been that to measure real affective disorders – still brought on today by the administrative procedures of universities – was more scientifically valid than artificially created situations. It was a rare occasion when he could present the same stimuli – individual words – to a number of subjects and objectively compare their reactions. He went further and in some cases the subjects agreed to retake the test after they had completed their reviews, thereby facilitating a comparison of *Fern- und Nahe-Wirkung* (long- and short-range effects – cf. Goldstein 1971a, 140 for the use of the term *Nahe-Wirkung*). In this way he was able to compare the reactions at the height of their trepidation and after their agitation had diminished. He was therefore able to chart the dynamics of the affective disorder and measure some of its effects. Although one might consider this experiment insensitive and attribute it to the arrogance of youth or to a scientist unconcerned with the personal feelings of his subjects, we know from Luria’s later work with brain-damaged patients, that this was certainly not the case. Furthermore one could argue that he was actually providing objective evidence of the harm rendered by such institutional devices.

Thirty students were examined by Luria and Leont’ev - nineteen were women, eleven were men. Of these eleven consented to the repeat experiment to examine the expected subsequent reduction in tension (*Entspannung*). [Luria’s use of German terms is unexplained. Another term used, *Entladung* – discharge, is also so widespread a concept that no special theory is called to mind]. A control subject, unaware of the purpose of the review, also took part. His responses remained calm throughout. The stimuli consisted of three groups, each stimulus being one word. It was considered to fall into one of these groups on the basis of its relation to the review. Critical words included ‘examination’, ‘commission’, and ‘roll-call’. Indifferent words were everyday words like ‘dress’ or ‘pipe’, and doubtful words included ‘student’, ‘constitution’ and ‘broom’. The stimuli were measured in four groups – the word following a critical word was measured

separately to measure the possibility of the reaction to the critical word producing an after-effect in the form of a diffuse affect. In the statistical tables and graphs the results are very clear and support the approach presented in Luria's introduction (cf. Luria 57ff for the stimuli and results).

Luria's account compares the reactions of the control and two typical subjects to the critical stimulus word "cleansing". The control subject exhibited stable and regular reactions. The behaviour of the notionally separate verbal and motor systems was completely co-ordinated. Their responses occurred simultaneously. "Precisely these properties of the regulation of the neurodynamic process are destroyed in the state of acute diffused affect" that the others manifested (51). The verbal responses were slower and sometimes stalled. "The affect provokes a functional lowering of the associative possibilities" (ibid.). Indeed "the failure of co-ordination ... begins with a collapse of higher regulation and ... disturbs the co-ordination with the motor sphere" (52). Luria refers to the "primitive forms" of associative verbal responses – not simply excitability and distortion but responses to incidental and accidental stimuli that are usually ignored. Meanwhile the excitation results in impulses that destroy motor co-ordination.

Luria considers it "a law of affective behaviour" that "in the affective state the motor setting has a tendency to be directly realized, the excitation does not meet with any delay, with any inhibition, and immediately proceeds to its terminus. That which is damaged is the restraining system, the barrier which inhibits the direct appearance of the motor act, and which enables the co-ordination with other reactive systems to occur" (55). Shortly after, in reference to a related experiment from 1927 he writes, "Recent analyses prove that behaviour in a state of affect causes the organism to revert to long past, primitive stages of development of the neurodynamic mechanism" (ibid. 68). Here, although the book does refer to the work of the pioneering neurologist John Hughlings Jackson (1835-1911) and others, such quotes are more consistent with the works that he was then familiar with, namely Freud's *Beyond the Pleasure Principle* and W.H.R. Rivers' *Instincts and the Unconscious* (1922). Both these works refer to the examination of the so-called "War Neuroses" and the revision of psychoanalytic theory that this engendered.

I conclude that what Luria published about this experiment in 1932, which he stated was based on his 1926 article (Luria 1932a, 47), is largely consistent with his approach in 1924. Some of Kurt Goldstein's terminology does appear, but it is not central to the experimental design. This reveals the astonishing conceptual level he had attained by 1924, and the enormous leap he had made in his experimental organization and assumptions. I think it also shows already the potential awaiting himself and Vygotsky when they began seriously to investigate both aphasia and child development.

III

The Russian Psychoanalytic Society in its Heyday

In his first report as Secretary of the Russian Psychoanalytic Society Luria presented an organization that was active and flourishing. The State Institute extended its work beyond that of the Children's Home and laboratory. It set up a series of lectures given by Ermakov, Wulff and Spielrein for physicians, educationalists, psychologists and students. Wulff's lectures were introductory, Ermakov's dealt with principles and psychotherapy, and Spielrein's course was titled "The Psychology of Subliminal Thought". A range of seminars was given. Others were reported to be in preparation – including "Research into Complexes" by Luria. Ermakov, Wulff and Spielrein directed Averbukh and Fridman's work in a newly opened outpatient department, and Ermakov and Spielrein ran a special children's outpatient department. There were twelve children aged 2 to 4, and younger children were expected at the Children's Home and Laboratory. The laboratory had collected many observations on the games and speech of children, together with observations of their sexual life (Luria 1924a, 258-9). A pamphlet on this research by Vera Schmidt was published in Germany in 1924 (republished as V. Schmidt 1969. Cf also W. Schmidt 1930). A series of books was published by the society under the imprint *A Psychological and Psychoanalytical Library*. It comprised translations of Freud, mainly by Wulff. This series was edited by Ermakov. His studies of Pushkin and Gogol were among the various other works that it contained (ibid. 260). 1924 was undoubtedly the high water mark of psychoanalysis in Russia.

On 15 November 1923 Sabina Spielrein spoke to the society on “Aphasic thinking and infantile thinking”. A number of writers of the psychoanalytic persuasion are convinced that Spielrein’s views lie at the basis of the ideas of both Luria and Vygotsky. Attempts to link her to Vygotsky have never made much sense as there is no evidence that they ever met, nor did he ever refer to her works. The suggestion by Etkind (1997, 174) that Vygotsky acquired the idea of internal speech from Spielrein is frankly absurd, given Vygotsky’s earlier reading of Alexander Potebnia (cf. Kharitonov 1991 and Nadia Kerecuk’s articles and forthcoming translation of Potebnia’s *Thought and Language*). Although Luria was a colleague of Spielrein’s at the State Institute, he never referred to her work either. Fortunately we can assess her talk since it had been published in Geneva before she returned to Russia. She did refer to Hughlings Jackson, Henry Head and other leading experts on aphasia, but neither Luria nor Vygotsky were interested in aphasia at that time. Nor did they have access to their principal works – Head’s *Aphasia and Kindred Disorders of Speech* was not published until 1926, and most of Jackson’s writings were relatively inaccessible until 1932. Spielrein’s article proposes that in infant speech and aphasia links between verbal language and the visuo-kinaesthetic subconscious are evident, and this latter link may be crucial for the development of language (Spielrein 1923). This was not an area of investigation for Luria. Of course Jackson was one of the few people whom Freud praised, both in *On Aphasia* (1891), and elsewhere (cf. Sulloway 1979). Several writers have attempted to make the link Jackson-Freud-Luria (cf. Solms & Saling 1986, Solms 2000). Although Jackson and Head appear to have helped Luria and Vygotsky finalize a framework for their ideas in the early 1930s, I would argue that this helped them consolidate their break with psychoanalysis (cf. V, iv, 146-7; VII, ii, 199-205 & iii, 216).

Throughout 1924 the Children’s Home suffered a series of crises. Although there were inspections and some hostile comment (cf. Marti 1976), Etkind’s access to various archives has provided an account, which, while still confusing, extends the parameters. Although there are those that blame the state for the closure, apparently the female teachers at the home did not get on with the director Ermakov, and protested at their lack of training in psychoanalytic techniques.

Eventually the home and the State Institute were separated. The staff were sacked at the end of 1924. Technically the home was still open, but in fact it was finished. In August 1925 both the home and the State Institute were formally dissolved. The society continued, though presumably decided not to publicly report these events to the international movement. Sabina Spielrein retired to Rostov-on-Don, but otherwise the members of the society continued to meet, sporadically after 1930 – the international movement received a report in 1930, and membership lists for 1931 and 1933. Allegedly it continued despite a hostile government until 1936, though the evidence for this is thin (Etkind 1997, 210-7).

Nevertheless 1924 was a busy year for Luria and the All-Russian Psychoanalytic Society. On 29 May Luria spoke on “Psychoanalysis as a System of Monistic Psychology”. Luria reported (1925a, 243), “The old experimental psychology was always idealistic and like a mosaic. Psychoanalysis alone had the courage to take two big steps towards a monistic ‘whole-psychology’, firstly by pointing to the erogenous zones as contributory factors in mental development (the fundamental principle of organic psychology), and secondly, by considering personality in its inter-relations with the environment”. As we shall see this is consistent with the published article (Luria 1925b) in that it attempts to examine both internal and external stimuli in their several manifestations. What he does not report is that through his examination of the external stimuli, he attempted to reconcile the materialism of Marxism with what he saw as the materialism of psychoanalysis.

We do not know for sure when Luria joined the Communist Party. Marti (1976, 216) states that the Kazan troika, including Luria, together with the Schmidts, allegedly related to the Bolshevik commissar V. V. Schmidt (1886-1940), Reusner, Kopp, A. B. Zalkind, A. K. Voronsky – indeed probably the majority of the society’s members – were also members of the Party. Unfortunately, although we know this to be mostly true, Marti provides no references. In Luria’s case we know he actively supported the ideals of the Revolution, but it has also been stated that he did not join the Party until 1943 (cited in Homskaya 2001, 113). More to the point in respect to his article, however, is the attempt by Kornilov to use Marxist philosophy to improve the theory and methodology of psychology. Although it is often assumed that Luria learned his Marxism from Vygotsky,

Kornilov would have certainly spurred him on. In January 1924 Kornilov gave a lecture on “The Dialectic Method in Psychology”, a year before the publication of Engels’ *The Dialectics of Nature*. In 1925 he edited a volume entitled *Psychology and Marxism* (see below), which included Luria’s article. The translation of his *Psychology in the Light of Dialectical Materialism*, though later (Kornilov 1930), includes most of the references used by Luria in 1925, extending to Feuerbach’s *Against the Dualism of Body and Soul, Flesh and Spirit*. On the other hand it is more than likely that Luria sat down and ploughed through the works himself, and then gradually clarified his ideas about the relevant aspects of Marxism. Although parts of Luria’s article were almost certainly written after his first presentation of its subject matter, he was prepared to discuss the issues again at the beginning of 1925 as part of a big public debate on psychoanalysis, Marxism and materialism.

Perhaps the Psychoanalytic Society felt the need to raise its materialist profile in the context of the de facto closure of the Children’s Home. Wulff became president and Ermakov became joint vice-president with Trotsky’s ally, Viktor Kopp. Luria remained on the committee with Kannabikh. Presumably this new committee organized the debate, though it is interesting that, as we have seen, Kornilov edited a collection which included Fridmann’s contribution to the debate as well as Luria’s. Luria undoubtedly played a key role in this, and we give his report below. It has not been translated before and providing a substantial extract from it makes an appropriate conclusion to this section, because it not only discusses the various theoretical currents, but also because it gives the flavour of the times.

The broadest of the discussions [initiated by the Society] was that concerning the philosophical-scientific bases of psychoanalysis. The Russian Revolution drew much attention to the scientific philosophy of Marxism and so-called dialectical materialism. Any scientific methodology, whose principles would always be philosophically based, was discussed from this standpoint, and the debate about psychoanalysis and Marxism was one of the most interesting. One side in the debate viewed psychoanalysis as a method based entirely on a scientific, i.e., natural-scientific, materialist foundation. Psychoanalysis appeared to these colleagues as a thoroughly monistic system, characterized by its dynamic and to some degree dialectical viewpoint. Some articles by holders of this view appeared

in the autumn of 1924. Indeed those of Al. R. Luria [Luria's standard self-reference], "Psychoanalysis as a System of Monistic Psychology", Dr. B. Fridmann, "The Psychoanalysis of Freud and Historical Materialism", are both in the collection *Psychology and Marxism* [(Kornilov 1925) - as was a chapter by Reusner. Inconsistencies between the real date and the printed date of publication were common. Luria's article is examined in the following chapter]. Others will shortly appear as a second collection, *Psychoanalysis and Materialism* (articles by Dr. M. Wulff, Al. Luria, B. Fridmann, W. Rohr, and others) [This was not published]. The other side in the debate took another position and stood firmly as the principal opponent of psychoanalysis. Its members asserted that psychoanalysis held idealistic assumptions, its main theories had little to do with materialism, and its metapsychology merged with metaphysics. The principal leader of this tendency, [V.] Iurinetz is unfortunately a thoroughly philosophically oriented man, rather than a natural scientist. He began the debate in the journal *Pod Znamenem Marksizma*. [His attack on psychoanalysis (Iurinetz 1924) was probably the major catalyst for these debates]. Some scholars, including Professor Fritsche, a noted student of the theory of art agree with him, and at the beginning of this year [1925] two debates took place.

One, on psychoanalysis and Marxism, took place in the Moscow Press House; the second, on psychoanalysis and the psychology of art, took place in the Communist Academy. At the first debate, which took place over two evenings and attracted many listeners, Iurinetz spoke against psychoanalysis, which in turn was represented by a number of scholars, mainly members of the Russian Psychoanalytic Society. Dr. M. Wulff, Professor Reusner, Professor Charasov, W. Rohr, Al. Luria, Dr. A. Zalkind, J. Schaffiz, Dr. Fridmann, Dr. Vnukov and others spoke in favour of psychoanalysis.

Since these debates some tendencies have differentiated themselves with regard to hitherto unnoticed philosophic adjustments within Russian psychoanalysis, we would like to elaborate on them here. Apart from the supporters and opponents of psychoanalysis, there are also scholars who accept an amended psychoanalysis, and others who are attempting to rebuild psychoanalysis along original lines. For example, a scholar like Professor Reusner, the noted Russian jurist and sociologist, belongs to the group. In his studies of the psychology of religion he completely accepts psychoanalysis, but he believes that some of the more recent theories of psychoanalysis, for example the metapsychology, depart somewhat from the earlier

position of psychoanalysis, and are difficult to reconcile with materialist philosophy. The other tendency is most marked in the article “Freudism and Marxism” in *Krasnaia Nov’* [1924, 4, 21], and other articles in specialist publications, by Dr. A. Zalkind. He holds the view that, in the dynamic and active stance of psychoanalysis, the person and the psyche are a unity of competing objectives, an insight of great value. This tendency believes that it is not necessary to understand the psychoanalytic theories of sex and libido. In Zalkind’s opinion psychic energy arises in the organism as a whole under the influence of the social environment. Only in cases where the organism is separated from the social environment, will the energy in the ‘lower’ courses be transferred and used for purposes of individual activity (e.g., all forms of sexual activity). But the main drives of human life are the social instinct and the drive for power, and Dr. Zalkind believes there to be a Russian “Adlerian reflexology”. This last deserves emphasis, since Dr. Zalkind has taken the trouble to translate the whole psychoanalytic system into the language of objective psychology and reflexology.

In Russia the influence of the so-called objective psychological tendency, (in particular physiology), based on the work of I. Pavlov, V. Bekhterev and K. Kornilov, is very large. The achievements of these objective schools have much of significance to offer for psychoanalysis (see the special article on this, [i.e., Luria 1926a –see IV,ii]). Therefore it is quite understandable that the tendency to yoke the two scientific courses together, and to join the psychoanalytic interpretation of psychic and pathological processes with the positions and mechanisms of the physiological school. Several psychologists and psychoanalysts are now pursuing their work along these lines (Luria 1925b, 397-8).

Chapter 3: Problems of Theory and Methodology

I

Vygotsky on the Significance of Speech for a Critique of Reflexology

The application of the combined verbal and motor method to word association was not simply an advance on its use in psychoanalysis, but was an implicit criticism of the failure of psychoanalysis to do objective research. In conversation with Michael and Sheila Cole in later years Luria said that despite “the arbitrary interpretations that were already characteristic at that time ... we felt that emotional states, complexes and the dynamics of mental life could be analyzed from an objective physiological point of view”. At the time he felt that psychoanalysis and objective research were compatible. But Cole then asked, “What indicators of emotional disturbance did you substitute for the Freudian approach?” Luria’s answer, “The combined motor method...” (Cole & Cole 1971, 79) is revealing. In retrospect he admits that his method replaced Freud’s, but the implication is that it also went beyond Freud’s theories. Luria may not have possessed a theory capable of explaining its findings at that time, but the method had something equally valuable. Namely, it raised issues about the nature of the workings of the psyche and human behaviour. It demanded answers and provoked Luria and then Vygotsky into seeking answers until they could provide a theory. This would take time until they arrived at a series of investigations in the late 1920s, which in turn were based on theories that they found themselves forced to come up with. Part of this process involved continual attempts to clarify where they had arrived in their theoretical positions together with an assessment of the problems and faults in the theories and methodologies of their contemporaries and seniors.

The Second Russian Psychoneurological Congress took place in Petrograd January 3-10 1924. Kornilov took his staff there from the Institute. Kornilov gave his speech on psychology and dialectical materialism. Other speakers included Bekhterev, Chelpanov and A. A. Ukhtomskii (1875-1942). This was obviously considered at the time to be a major event. Making his debut at the Congress was an educational psychologist from the provincial town of Gomel’. Who actually

invited Lev Semenovich Vygotsky (1896-1934) to speak is not known. (For Vygotsky's early career cf. Vygodskaja & Lifanova 1996/1998, also Yaroshevsky 1989, Van der Veer and Valsiner 1991). He gave three speeches, but he is mainly noted for the speech *Methods of Reflexological and Psychological Investigation*. Luria remembered that "when Vygotsky got up to deliver his speech, he had no printed text from which to read, not even notes. Yet he spoke fluently, never seeming to stop and search his memory for the next idea. Even had the content of his speech been pedestrian, his performance would have been notable for the persuasiveness of his style. But his speech was by no means pedestrian" (Luria 1979a, 38). Luria was so impressed that "he set about persuading Kornilov ... to immediately invite this person, known to no one, to work in Moscow at the [recently renamed] Institute of Experimental Psychology. Lev Semenovich accepted this invitation" and began work there in the autumn (Luria 1977, cited in Vygodskaja & Lifanova 1996, 74/ 1998, I, 58. Cf. also Luria, cited in Cole & Cole 1971, 82). No contemporary has disputed Luria's account, though the suggestion that he was pushing at a relatively open door has also been implied. What led Luria to take this course of action? Although technically Luria was to be senior to Vygotsky in the department, in hindsight, as regards ideas and influence, it might appear as if Luria had headhunted his own leader!

Although we don't know precisely what Vygotsky said in Petrograd, he published an article in 1926 under the same title. The content probably bears some relation to what Vygotsky actually said – though how close the relationship was remains speculative. The contents of the article would certainly be sufficient to explain the impact the speech had on the congress. In it, he investigated the basis of reflexology in stimuli and reflexes, examined its potential, and the inconsistencies of its leading practitioners Pavlov and Bekhterev. It should be noted that Pavlov liked to distinguish his work from Bekhterev's reflexology, and preferred to call it physiology or the science of behaviour, but in the writings of Vygotsky and others it was assimilated under the term reflexology. Vygotsky widened its scope, raising issues that it had failed to address, and in that sense seemed to be attempting to transcend it. In so doing, further methodological issues arose, both explicit and implicit. But in the article these are not yet developed. It is easy to see why attention became focused on Vygotsky. The criticisms he raised were felt by many

and often implicit in their work. Having put them together forcefully, he inevitably appeared as someone who offered the promise of resolving the new issues that thereby arose. Others would surely want to see what he produced.

The faults he finds with reflexology arise from its failure to address the problem of speech. "That speech has to be considered a system of conditional reflexes hardly needs any discussion: for reflexology it is almost a truism" (Vygotsky 1994a, 29). Surely this view could lead to the merging of the methodologies of reflexology and psychology? Unfortunately the reflexologists could not include the interrogation of a subject in an experiment, as they believed it involved the latter's subjectivity and processes that were not manifest. Bekhterev (1932, 61-2, 200), a softer target in this respect, provided the paradoxical conclusion that "From the standpoint of reflexology subjective investigation is permissible only on oneself". Vygotsky (1994a, 31) notes that Bekhterev followed Sechenov in demonstrating "that thought is ... an inhibited reflex, a reflex that is non-manifest," and this is particularly true of verbal thinking. "Thus, either we refrain from the study of human behaviour in its most essential forms or we introduce the obligatory registration of these non-manifest reflexes into our experiments. Reflexology has to study both thought and the whole mind if it wishes to understand behaviour" (31-2). Inhibited reflexes are also objective.

[We can study them, because] reflexes do not act separately ... but club together in complexes, in complex groups and formations that determine human behaviour. The laws of composition of reflexes into complexes, the types of these formations, the sorts and forms of interaction within them and the interaction between whole systems – all these questions have paramount meaning for the most acute problems of the scientific psychology of behaviour. [Already we can speak] about the undeniable interaction of different systems of reflexes, about the *reflection* (interpenetration) of some systems on others, and we can even in general provide a preliminary clarification of the mechanism of this reflection. *The response part of each reflex (movement, secretion) becomes itself a stimulus for a new reflex from the same system or another system* (32).

Vygotsky chooses to italicize the final sentence of this paragraph. But as a precursor of the work that he and Luria subsequently undertook the whole passage could be highlighted.

He continues (*ibid.*), “Although I never came across such a formulation in any of the works of the reflexologists, its truth is so obvious that it is evidently only omitted because it is tacitly implied and accepted by everybody”. Furthermore, given “the gigantic, colossal role that precisely the mind (that is, the non-manifest group of reflexes) plays in the system of behaviour, it would be suicidal to refrain from exposing it through the indirect path of its reflection (bearing) on other systems of reflexes” (33). The Wuerzburg school’s use of the self-reporting of subjective consciousness was unscientific in that it placed the subject in the role of the observer, but there were several ways in which verbal responses could be incorporated within experimental procedures that avoid this (*cf.* 43-4). To refuse to incorporate the objective phenomena of the mind, even though its content was subjective for the subject, was a dereliction of scientific responsibility. Methodologically, “in essence, dualism is the real name of Academician Pavlov’s and Bekhterev’s point of view” (39).

Thus far, everything that has been stated by Vygotsky is consistent with what Luria was doing in his experimental practice. Vygotsky’s formal methodological legitimization of this approach would have been sufficient grounds not only to encourage Luria’s continuation of this work, but also to persuade him of the value of such a potential ally. But Vygotsky expanded the scope of his arguments much further. Not only did Vygotsky discuss the examination of reflexes within the unconscious and consciousness, but also the social aspects of thinking and speech. These were his lifelong concerns, stimulated by his previous linguistic studies and experience in educational psychology.

Vygotsky could see the amusing side of his exposition as well. “To claim that consciousness too has to be understood as a reaction of the organism to its own reactions, one has to be a bigger reflexologist than Pavlov – so be it...” (40). He aligned himself here with members of the Gestalt school who accused psychology as a whole and behaviourism in particular of the same sin: failing to develop a

single accepted methodology (42). It might therefore be possible to see Vygotsky as an outright critic of reflexology, but this would only be true from a methodological viewpoint. He wanted physiology to be an intrinsic component of psychology, to ground it materialistically. Pavlov continued to see psychologists as subjective and did his best to ensure their ideas did not contaminate physiology to the extent of fining his laboratory staff who used psychological terms in defining their animal subjects' states of mind. At this time he was reporting on the creation of experimental neuroses and eventually moved further into psychiatry (Pavlov 1941). Vygotsky criticized reflexology from within, pointing out the logical contradictions of its failure to engage with higher mental activity in humans. Thus it was the concept of "a reflex of the second order" that Pavlov spoke of in 1910 (Pavlov 1928, 105), that enabled Vygotsky to speculate further.

Stimuli and reflexes were crucial for Vygotsky to enable him to raise the issues of consciousness and the unconscious. But this required far more than a simple reflex act. As we have seen "*the response of each reflex (movement, secretion) becomes itself a stimulus for a new reflex from the same system or another system*" (Vygotsky 1994a, 32). This is why, in a psychological interrogation, a subject is no longer a witness, an outside observer, but an inherent part of the experiment. The response cannot be treated simply as a conversation, "but as *a system of stimuli* with an accurate registration of each sound" (34). "Indeed, in man, a group of reflexes that we should strictly call the system of 'reflexes of social contact' (A. B. Zalkind) easily stands out. These are reflexes to stimuli that in their turn can be created by man... These reversible reflexes, that create the basis of consciousness ... also serve as the basis for social interaction, and the collective co-ordination of behaviour, which incidentally points to the social origin of consciousness". From the whole variety of stimuli this group of social stimuli clearly stands out, because these stimuli are reversible. Hence the subject can determine *his* "behaviour *in another way* from all others... Speech is on the one hand a system of reflexes of social contact and on the other hand primarily a system of reflexes of consciousness, that is, for the reflection of the influence of other systems" (35). Vygotsky continued, "that is why the key to the resolution of the problem ... of the recognition of another person's mind lies here. The mechanism of consciousness of the self (self-consciousness) and the recognition of others is the

same; we are conscious of ourselves only because we are conscious of others... We are conscious of others only to the extent that we are another to ourselves, that is, to the extent that we can perceive our own reflexes as stimuli" (35-6).

Consciousness itself "is correlative activity within the organism itself, within the nervous system, correlative activity of the human body with itself" (41). "Consciousness is an indisputable fact, a primary reality, a fact of the greatest significance, and not a secondary or accidental one... As long as the new psychology does not make both ends meet, the problem of consciousness will not be stated clearly and fearlessly and it will not be solved in an experimentally objective way" (ibid.). Luria singled out this aspect of Vygotsky's talk in his autobiography (Luria 1979a, 38).

Although many of these relationships of language and consciousness were implicit or explicit in the writings of Herder (cf. Appendix I), Vygotsky reintroduced and amplified them within the framework of the reflexological approach to psychology. It immediately resonated with at least some of his audience. Vygotsky's talk foreshadows much of his work, especially his later major writings. The connections made here between the growth of self-consciousness and its social origins were to provide a strong link with the Marxist approach and hence Vygotsky and Luria's joint work of the late 1920s. At this stage however Luria did not attempt to formally develop this area of work, but continued his research into another area that Vygotsky raised, namely, the interaction of reflexes and groups of reflexes.

Finally, it should be pointed out that Vygotsky's whole approach to language, consciousness and self-consciousness is implicitly infused with the concept of feedback. Vygotsky's future colleague N. A. Bernshtein formally introduced this concept into physiology in the 1930s when he applied it to the neurology of body movements (cf. Kozulin 1984, chapter 3). It had long been implicit in the linguistic philosophy of Herder and his successors. It is commonplace today, when seeking a response, to ask for feedback. But it had also been implicit in the work of Sechenov, and Pavlov's own characterization of conditional reflexes as 'signals' helped lay the basis for its formal introduction. In the early 1920s

physiologists were hoping to move beyond Pavlov's concept of the 'reflex arc', where reflexes were treated as one-way transmissions. A reflex arc is a chain composed of an afferent nerve with a 'receptor' (receiving apparatus) and 'analyzer', a central part described as a 'conductor' and 'connector', and an efferent nerve acting as an 'effector' (cf. Pavlov 1928, 242-3). Pyotr Kuzmin Anokhin, then a researcher for Pavlov, may also have attended Vygotsky's talk and may have met Luria there for the first time (K. Anokhin 2001, personal communication). In 1935 in his exclusively physiological version of the concept 'functional system' Anokhin assumed that such a system was self-regulatory, i.e., dependent on feedback. The theoretical consequences of an as yet implicit feedback approach to speech and consciousness would also in time be made clear in the work of Luria and Vygotsky in the study of the interactions and interpenetration of these groups of reflexes. Luria's employment of the subjects' hands in his combined verbal and motor procedure made certain assumptions about their movements (cf. Luria 1929a, 173; 1932a, 175). Indeed, in a paper written in 1928, he made explicit use of some of Bernshtein's research (Luria 1929a, 135).

II

'Psychoanalysis as a System of Monistic Psychology'

Psychoanalysis as a System of Monistic Psychology, written and probably published in 1924 – despite the official publication date, concentrates, not entirely uncritically, on the materialistic dimensions evident to Luria in psychoanalysis. Here, as we would expect, the role of reflexes, stimuli, and the objective quality of registered verbal responses in psychological experiments is assumed. Vygotsky focused on seeking recognition that the physiological side of psychology should not be separated from speech, consciousness and thinking – and hence society. Luria's article can be seen as complementary in that he too went beyond what he termed the old scholastic psychology, by introducing both Marxism and psychoanalysis into the debate. This could be considered unwise, as he himself would admit later. But he was not alone in this. It may be that he only grasped a portion of psychoanalysis and misconstrued its other elements. If so, it was a common failure both of Russian scientists and the Russian *intelligentsiia*,

including not only many of his colleagues such as, to some extent, Vygotsky, but also Trotsky.

Luria rehearsed again his arguments against the old psychology. It studied psychological phenomena in discrete, isolated elements, mechanistically and with no attempt to understand them in motion, dynamically - or how they operated together within the personality of the whole human being. Correctly he saw the old psychology as being incompatible with the scientific materialism elaborated by Marx and Engels. He criticized the atomistic nature of the old 'mosaic' psychology.

Of course a system of psychology built in this way was not even able to begin the study of something such as an integrated neuropsychological process, the real basis of human activity that characterizes man's behaviour, motives, responses and so forth... [It] had long since given up studying the whole man, to say nothing of man as a creature shaped by the specific conditions of his socio-economic and, above all, his class situation (Luria 1978b, 9).

He saw psychoanalysis as offering a dynamic scheme that appeared, at least in part, to be consistent both with this desired approach and with Marxism. Although he would later judge the grander claims of psychoanalysis to be both reductionist and metaphysical, at that time no sensible scientist could afford to ignore it completely. We should not dismiss Luria's work as a youthful aberration, but instead analyse it in terms of what he could see of value in psychoanalytic approaches, and how they would work as a catalyst to his ideas. If, like Luria or Vygotsky in the 1920s, one was trying to build a genuinely scientific psychology, one could not start from scratch. Many of the available building blocks were cracked or flawed, but some of them offered a temporary advance, and the opportunity to create something better.

Psychoanalysis "proposes a monistic and dynamic approach to the personality. Instead of studying things in isolation, it would study continuous processes that reflect the organic continuity between the life of the child and the mind of the adult human being" (10-11). Luria noted "the fundamental similarity between the

respective approaches of psychoanalysis and reflexology” (41, n. 59). Psychoanalysis distinguished two types of stimuli affecting the human organism and, in turn, eliciting reactions. These were “external stimuli, coming from the biological and social environment, and internal stimuli, originating in physiological processes taking place in the body and its various organs” (18). Although, in principle, psychoanalysis did not distinguish between the two, it focused on the internal stimuli, otherwise known as drives (*ibid.*).

Psychoanalysis shifted the theory of mental phenomena to an entirely new plane, that of the organic processes taking place in the human organism as a whole. It thereby made a decisive break with the metaphysics and idealism of the old psychology, and has laid the first solid foundation (together with the theory of human responses and reflexes) for a materialist, monistic psychology that takes a positive approach to the mind of the whole person (30). It has taken two major steps: it has affirmed the inter-relatedness of individual mental functions, and it has reintegrated the mind into the overall system of organs and their biologically determined activity (30-1).

Although it is easy to construe this as it stands as a form of biological determinism – making it indeed a complementary approach to Vygotsky’s emphasis on cognition, Luria was not a biological reductionist. He concluded:

If the system of psychoanalysis is to measure up better to the requirements of dialectical materialism, however, it must develop a fully dynamic dialectic of mental life and take a third step toward a holistic approach to the organism. It must now integrate the organism into a system of social influences. Only then will the theory of neuropsychological activity advance from mechanical materialism to dialectical materialism. It is with these aspects of psychoanalysis that we shall be dealing in a later work (31). [To make this passage more coherent I have included note 62 within the text].

Luria is here admitting that more work needs to be done before the physiological, neurological and biological elements of psychoanalysis can be linked to the social and economic elements of Marxism. I think therefore Vygotsky, after his later change of mind about psychoanalysis, missed the point when he criticized Luria’s

article on the grounds of eclecticism – for Luria had clearly stated that what Vygotsky called a “monstrous combination” had yet to be fully and satisfactorily effected (cf. Vygotsky 1997b, 259). But it was reflex theory itself, which Vygotsky supported that elicited this eclecticism. Differing sets of internal, external and social stimuli almost inevitably required different sets of explanations.

On the other hand, Vygotsky’s criticism is correct from the point of view of the respective methodologies. But even here Luria recognizes that much of Freud’s work is speculation, as Freud himself freely admitted. Luria cites Freud as hoping that science would eventually provide the research that would support his theories. Luria criticizes “the ambiguous formulations presented in psychoanalytic systems” and “the subjective terminology which Freud himself says was provisional and needed to be replaced by an organic terminology” (Luria 1978b, 21; cf. also 14, 23, and 36, notes 29 and 30). He seems to go beyond Freud’s interests and call on the research into the endocrine system (4, 21), something to which Freud, for all his claims, paid scant attention. If one can judge by the minimal references to hormones and glands in the *New Introductory Lectures on Psychoanalysis* (1933), which contains the majority of his remarks on the subject (Freud 1974), he was dismissive of any significant independent role for the endocrine system. Luria’s approach to psychoanalysis thus involved elements of faith, pragmatism, and criticism.

The term ‘monistic’ in his title refers to the work *The Development of the Monist View of History* by G.V. Plekhanov (1856-1918), the ‘father’ of Russian Marxism. Under the Tsarist regime he had used the term ‘monism’ as a term representing Marxist philosophy that would pass the censors. It represents an integral materialist view of nature, humans and their thinking. Plekhanov was also the first to use the term ‘dialectical materialism’ as an alternative to Marx’s term ‘materialist dialectics’.

Feuerbach’s work, *Against the Dualism of Body and Soul, Flesh and Spirit*, also impressed Luria. “Feuerbach brilliantly anticipated many of the concepts of the new psychology. His arguments for a monistic approach to the individual, about

feelings, about the relationship between cerebral activity and the activity of the organs of the body were altogether a classic prototype of a sound and profound approach to the problem of individual personality” (Luria 1978b 33, n.12). Although Ludwig Feuerbach (1804-72) was notorious in Marxist circles for his abstract and ahistorical conception of individuals, in this work Luria’s judgment is correct (cf. Feuerbach 1904, 340).

Luria’s first attempt to come to terms with Marxism was only partially successful. He referred to works by Marx and Engels that are relevant to the psychology of the personality. He had clearly gone to great lengths in his studies – referring to original works in German including the original 1895-6 publication in *Neue Zeit* of Engels essay *The Part Played By Labour In The Transition From Ape To Man*. Surprisingly the English translation (Luria 1978b) omits the quotations from Marx’s *Theses on Feuerbach*, Engels’ *Anti-Duehring*, *Ludwig Feuerbach and the End of Classical German Philosophy* and other works he used to support his methodological criticisms of idealist approaches to science (for the Russian version see Luria 1994). He does, however, praise the dialectical materialist methodology, which recognizes that material conditions ceaselessly change – in distinction to what he calls “metaphysical materialism”. “This is where Marxism introduces its dynamic view of things and events as a necessary principle and draws a firm line between itself and the static, metaphysical view of things that tends to see phenomena as discrete, isolated, unchanging essences, not as processes” (Luria 1978b, 5-6). “With respect to the problems of the mind, this approach does postulate an integral concrete person as its subject matter, not isolated functions of the mind, as had been the practice in general psychology” (11-12). Here, I believe, is the basis of Luria’s attempt to link psychoanalysis and Marxism. Material *processes* were central to both of them. On the other hand, Luria’s remark that “in Marxist theory the mind is conceived as a reflex to social stimuli” (33, n.11), explains why reflex theory in itself encouraged the above-mentioned eclectic approach. These remarks also show that not only had he not yet had access to Marx’s comments on the role of language, and could not yet use Marxism’s full potential, but also that he and his fellow scientists needed to expand their horizons.

Freud's work relied too much on speculation, and he failed to engage in experimental research to validate his ideas, thought Luria, but at least he approached the personality as a dynamic, material unity. And, of course, he also shared many recognizably scientific approaches and concepts of this dynamic, material unity with Russian physiologists. The monistic potential of psychoanalysis became evident through the dynamic processes of this psychological unity. For instance, the psychic energy associated with mental trauma may be 'converted' to somatic energy in neuroses or hysteria (16-17). Freud described the concept of drive as on the "borderline of the mental and the somatic". Drives were "an active ingredient of all the mental manifestations of the individual, selecting from among the multitude of stimuli only those that are suited to it, and in this way enabling the organism to actively adapt to the environment" (19-20). It was well documented by psychologists that associations, memories, and observations – including the statements of witnesses – were all affected by subconscious processes and preferences (*ibid.*). Even the concept of pleasure could be seen not as a subjective abstraction, but as the organism's measurable relief from the bombardment of so many internal and external stimuli. Such findings make "mind an integral part of the organism's system. It can hence no longer be studied in isolation" (21). They were also in line with Luria's view that psychology was a "biosocial science" (4). Thus "psychoanalysis is heading in the direction of a monistic theory of individual behaviour" (15).

Here Luria makes plain what attracts him to psychoanalysis. Today such an approach would be considered a one-sided view of psychoanalysis, an interpretation driven by his own scientific pre-occupations. We have to remember that Freud was primarily a therapist and a theorist of therapy. Admittedly his early work had been in the fields of biology and neurology, but he accepted from the beginning in the 1890s right up until the 1930s that there would never be enough known about the workings of the mind in his lifetime to explain what he wanted to know. Hence his justification for the role that speculation openly played in his theorizing (*cf.* Stewart 1969, 30-2). In this light it was inevitable that any scientist excited by psychoanalysis would willy-nilly have to interpret it and develop it in the light his own scientific research and experimentation. It is a logical contradiction to develop science from a largely 'mythological' position; yet

whether recognized as such or not, it is nevertheless often used as a catalyst to scientific development. W.H.R. Rivers' classic work, *Instincts and the Unconscious* (1922), is a case in point. The so-called 'war neuroses', the effects of the trauma of battle, clearly showed that the unconscious could not be viewed simply as the province of the libido as many of the responses manifested were instantaneous and could not be ascribed to the suppression by a superego.. Rivers died before any formal break with psychoanalysis could occur, but Ernest Jones' review certainly disassociated the movement from the first edition of the book (Jones 1920). Freud's response to arguments such as those of Rivers was that they had misunderstood psychoanalysis (Freud 1921, 1-4). As we shall see, *Beyond the Pleasure Principle*, though affected by the debate, subsumed the arguments almost without mention within a very unorthodox speculative solution.

Vygotsky's later comments (1997b, 262), that Freud was critical of Marxism, and that "not a single psychoanalytic journal would, of course, publish" these papers of Luria and his associate Fridmann, might be true. But, as we have seen, the *Internationale Zeitschrift fuer Psychoanalyse*, under the overall editorship of Freud, did publish Luria's account of the 1925 Moscow debates. All this is beside the point, however. Luria's article was designed for a Russian audience that was Marxist. It could be said that, in some sense, he was attempting (unknowingly) to fulfil Trotsky's request to Pavlov. When, on the other hand, Luria published in psychoanalytic journals, he attempted to present the ideas of Russian physiological psychological scientists to the psychoanalytic movement. The symptoms of naïvety, eclecticism and misrepresentation of psychoanalysis and Marxism should be seen for what we, with the luxury of hindsight can see, namely, that Luria was attempting to create a new more comprehensive, and more scientific, psychology.

When Luria first lectured on the above topic he was not yet 22. He thought he was improving psychoanalysis, and leading it into more objectively scientific realms. His conclusion (already quoted), that further steps should be undertaken, was intended seriously. He stated that his article constituted the first chapter of a book entitled *Principles of Psychoanalysis and Modern Materialism*, and he also hoped

to discuss the relevance of Alfred Adler's theories for social psychology (Luria 1978b, 3, 39 n.48).

When Luria spoke above of drive "selecting from among the multitude of stimuli only those that are suited to it, and in this way enabling the organism to actively adapt to the environment" (ibid. 19-20), he left his own interpretation of the role of social stimuli unclear. In Freud's early so-called 'Project [for a Scientific Psychology (1895)]' "the regulation of the flow of drive energy was the main function of the psychical apparatus" (Stewart 1969, 193). Luria cited Ukhtomskii's theory of 'the dominant', whereby centres of great excitation tend to attract and subsume the energy of non-dominant centres of excitation, thus providing a way for the organism to choose between stimuli. Luria's use of it to account for the evolution of the primacy of the genital over other erogenous zones (Luria 1978b, 40 n. 51) suggests that his interest in the systemic and neurodynamic aspects of psychoanalysis did not yet fully incorporate a pro-active role for the higher mental functions. Perhaps this would be resolved in his future investigation of Adler.

But the prospects were not encouraging. On March 10 1909 these issues had arisen at a scientific meeting of the Vienna Psychoanalytic Society. Alfred Adler gave a talk on the psychology of Marxism. As a member of the Austrian Social Democrat Party and allegedly an acquaintance of Trotsky, Adler approved of Marx's work. Even in terms of psychology, Marx saw "*the primacy of instinctual life*", by which he presumably means the basic human needs, which capitalism failed to satisfy. (Those very needs which the student Luria was driven to investigate!)

Their gratification is achieved indirectly by aggression, encompassing the conditions of production [i.e., class war]... [After the achievement of socialism], on a somewhat higher level of civilization, there appear altruistic ideas such as sympathy, charity, tenderness, and modesty, which are henceforth to rule the world. Psychoanalysis, however, has shown us that these 'ideas' are neither innate nor derivatives of a moral sense, but are built up of opposing impulses, which stem directly from the life of instincts. They are *reaction formations, which bring about*

an affective state that expresses itself as sensitivity (to debasement, degradation, ultimately to soiling) (Nunberg & Federn 1967, 173).

So much for social relationships, love, nurturing, co-operation or Kropotkin's 'mutual aid' – let alone the possibility of the development of rational behaviour! Reaction formations, it was agreed, was also the appropriate term to describe the formation of opposing classes under capitalism.

In response to the comments of this meeting, which clearly calls for dramatisation, Adler rejected suggestions that Marxism was a religious substitute, or possibly a neurosis. I have to confess I failed to understand the relevance of Freud's reference to the 'spinach of the Alps' (cow pats), but he did make the sensible comment that "Adler has failed to offer us any evidence of our line of thought in Marx. Rather Adler has tried to present the psychological foundation for Marxist positions" (ibid. 175). He suggested that Adler should continue his work and publish it. Despite Adler's conclusion that he wished "to stress that Marx's entire work culminates in the demand to make history *consciously*" (ibid. 178), one is left with the abiding impression that psychoanalysis would not countenance explanations of human behaviour, unless they derived from instincts. Or, as Freud put it in 1933, "strictly speaking there are only two sciences: psychology, pure and applied, and natural science" (Freud 1974, 179). Yet in the same work he said, "the theory of instincts is, so to say, our mythology. Instincts are mythical entities, magnificent in their indefiniteness. In our work we cannot for a moment disregard them, yet we are never sure that we are seeing them clearly" (ibid. 95). Thus the problem of combining Marxism with psychoanalysis has always been a fraught one due to the latter's self-proclaimed reductionism. Hence the criticism that Isai Sapir later made of Wilhelm Reich. Though Reich said that he espoused Marxism and the cause of the working class, and was to be expelled from the psychoanalytic movement, his scientific position remained reductionist (cf. I. D. Sapir 1929-30: 1929, 940; 1930, 145-6). During the mid-1920s Sapir became a friend of Luria's, and undoubtedly argued the same point forcibly with him.

There are alternate theories regarding how the organism selects between competing stimuli. One of these is relevant both for the following section, and for

the subsequent development of both Luria and Vygotsky - namely the position of Herder. As I argue in Appendix I, his views on human nature and language greatly influenced Marx, among many others. In *Ideas on the Philosophy of the History of Humankind* (1784-9), he argues that humans are not specialists and experts like other animals. Although they do have instincts, these do not dominate human behaviour in the same way, because, as generalists, humans have to vary their activities more. “Of all the instincts that imply *a relationship with others* [my emphasis-MH], the sexual instinct is the most powerful... Sex, like everything else about man, was designed, accordingly for voluntary control” (Herder 1969b, 268). The neurologist Kurt Goldstein (1878-1965) cited similar remarks by Herder to argue against reductionist approaches to instincts (Goldstein 1939, 478). In 1925 Vygotsky also cited Marx’s version of Herder in *The Psychology of Art* (1987, 78-9), “by placing excessive emphasis on the role of the unconscious, psychoanalysts completely negate that of consciousness. According to Marx, [in *The German Ideology*], consciousness forms the sole distinction between humans and animals. ‘The early human is distinguished from sheep only by the fact that with him consciousness takes the place of instinct, or that his instinct is a conscious one.’ [Marx & Engels 1976, 44]”. This approach, which Luria would shortly meet with in Vygotsky, also offered a dynamic view of what would become commonly known as neuropsychology, but would additionally introduce a much more significant and active role for the higher mental functions.

As we shall see in the following section, Vygotsky and Luria were quite capable of using contemporary neurological theories, other than Ukhtomskii’s, to explain the organism’s choice of stimuli to respond to. But I do think the indirect influence of Herder helped eventually to set the tone of their own responses.

III

‘Consciousness as a Problem in the Psychology of Behaviour’

On October 19 1924 Vygotsky gave a talk at the Institute in Moscow, attended by his new colleagues. It amplified what he had said about consciousness and language in his talk in Petrograd. It was published in 1925. *Consciousness as a*

Problem for the Psychology of Behaviour began with a long quotation from Marx's *Capital*, volume 1 (Marx 1976, 284). Marx wrote:

A spider conducts operations which resemble those of a weaver, and a bee would put many a human architect to shame by the construction of its honeycomb cells. But what distinguishes the worst architect from the best of bees is that the architect builds the cell in his mind before he constructs it in wax. At the end of every labour process, a result emerges which had already been conceived by the worker at the beginning, hence already existed ideally. Man not only effects a change in the form of the materials of nature; he also realizes his own purposes in those materials. And this is a purpose he is conscious of, it determines the mode of his activity with the rigidity of a law, and he must subordinate his will to it.

As is shown in Appendix I, this famous quotation, which Vygotsky used on several occasions, was taken by Marx from Herder. Apart from the embodiment of humanity in the architect, everything else can be found in Herder's *Essay on the Origin of Language* (1969c). Vygotsky's own works show no evidence of his ever having read Herder, though he would have known Herder's ideas in some form indirectly through the work of A. A. Potebnia, whom he did study. Although Marx does not mention language in this quotation, Vygotsky had no problem linking this epigraph to language.

Vygotsky wrote that he had "merely outlined ... some very preliminary ideas" (Vygotsky 1997c, 79). The February talk had been a forthright critique and theoretical amplification of the practice of reflexology and had considerable support. Here he was putting forward his own case, a much more important operation. But his introduction was equally forthright, another clarion call. After listing several scientific approaches applied to animals, humans and indeed the world, all of which could be found in Bekhterev's magnum opus (cf. Bekhterev 1932), he said that there was a notable absence. There was not a single law of the psychology of human behaviour that formulates the relationships or interrelationships of phenomena that are unique to human - as distinct from animal - behaviour. How was one expected to bridge the gap between the conditional reflex and the discovery of the laws of relativity? "The disparity between the roof

and the foundation, the absence between them of the building itself, easily demonstrates how early it is to formulate fundamental principles on the basis of reflexological material and how easy it is to take laws from other areas of science and apply them in psychology” (Vygotsky 1997c, 64). How simple and yet how unscientific, but how prevalent a practice.

Although Vygotsky’s analogy was not uncommon in psychology, it also echoes the state of Russia: a backward country with mass illiteracy, put back decades by wars, invasions, and civil war promoted by its capitalist opponents. Yet a country recovering and hoping in time to create a new form of society. Vygotsky’s comments of the roof and the foundations of a building seem to me to reflect the contemporary desires to transform society and science and to spread both literacy and scientific understanding throughout that society. It is more than a narrow scientific dispute; it reflects the hopes and expectations of a people and its *intelligentsia* hoping for a new society.

Certainly psychology needed to distinguish humans from other animals. “Man is by no means a sack of skin filled with reflexes and the brain is not a hotel for conditional reflexes that happen to drop in together” (cf. *ibid.* 66). Here Vygotsky is commenting on a notorious remark by the leading Bolshevik, Nikolai Bukharin (1888-1938), from his recently published work, *Historical Materialism*. “If we examine each individual in his development”, said Bukharin (1926, 98), “we shall find that, at bottom, he is filled with the influence of his environment, as the skin of a sausage is filled with sausage meat. Man ‘is trained’ in the family, in the street, in the school. He speaks a language, which is the product of social evolution... Each individual at bottom is filled with a social content”. Although the comment about the social evolution of language shows that Bukharin was not entirely a generalizing reductionist, that he was aware of certain specific psychological mechanisms, Vygotsky felt he had to challenge him. It epitomized his comments about the application of approaches and formulas from other areas to psychology without thinking through their relevance.

Similarly biology was used to devour sociology, and physiology psychology. Vygotsky argued that it was wrong to apply Pavlov’s law of the extinction of

conditional reflexes to humans. Over-stimulation of one area in animals would eventually lead to loss of reaction, yet no one suggested applying this theory to education. Again, Ukhtomskii's law of the dominant – that centres of excitation attract the excitation of other non-dominant centres – does not necessarily apply to humans. In man “each extraneous stimulus diverts and weakens attention” (Vygotsky 1997c, 65). It could be argued that Vygotsky was, strictly speaking, not comparing like with like, but that was also the point he was making. Humans have behaviours that are different. Psychologists had to struggle for their own scientific space, and could not allow their ideas to be reduced to such simplifications. In retrospect Vygotsky would almost certainly have applied his comments to psychoanalysis, but he was not ready for that. For an article that Vygotsky described as tentative, the taking on of both a leading politician and the ‘old psychology’ was perhaps enough.

Nevertheless, as Charles Sherrington (1857-1952) recognized, “the nervous system works as an integrated whole”, and this, said Vygotsky, “should form the basis for a theory of the structure of behaviour” (66). Luria agreed with this and had also referred to Sherrington. He also agreed that reflexologists had only studied a narrow range of behaviour. He would have accepted without question the proposition that “we must not study reflexes, but behaviour – its mechanism, composition and structure”(ibid.). So far reflexology had only studied the salivary, and defensive motor reflexes and conditional reflexes of the first or second order. Yet “consciousness should not be viewed as a second series of phenomena”. This was the crux of the issue for Vygotsky. “Consciousness is the problem of the structure of behaviour” (67) was his method of approach. This increased the theoretical horizon of any examination of the nervous system. Of course, Freud had examined consciousness in relation to the environment and the unconscious, so even if Luria had adopted a reductionist view, he could also accept Vygotsky's position. As we saw in section I of this chapter, this aspect, above all, is what he remembered most.

The detailed argument was new. Firstly, Vygotsky pointed out, following Marx, that humans did not simply adapt to an environment, to stimuli, but adapted the environment to suit their own purposes – here he reviews the passage on the

architect and the bee. Secondly, following Sherrington, he sees the outcome of the struggle of different groups of receptors for the common path as involving the collaboration of both major and minor reflexes. Although a struggle is involved, the singleness of action supports the view that normally “the reflex is an integrative action of the organism” (Sherrington, cited in *ibid.* 70). Thirdly, Pavlov used the term ‘chain reflex’ to describe a sequence of reflexes, but once we raise the possibility of the involvement of various systems of reflexes, i.e., a chain whose reflexes transfer between systems, we can apply it to our understanding of consciousness. “The capacity of our body to be a stimulus (through its own acts) for itself (for new acts) is the basis of consciousness” (71). “The psychological unconscious represents those reflexes that are not transmitted to other systems. There may be endlessly varied degrees of awareness, i.e., of cooperation between the systems connected to the mechanism of the active reflex. To be conscious of one's experiences is nothing other than to have them as an object (stimulus) for other experiences. Consciousness is the experience of experiences...” (*ibid.*) Thus “consciousness is always an echo, a response apparatus” (72). Thus far Vygotsky has suggested how physiology can be used to include consciousness within an integral system. But he also suggests extending its role from that of a mere echo.

“A circular reaction ... feeds its own reflex back into the organism via the centripetal currents that originate in the process, and ... this mechanism lies at the basis of consciousness”. But “a circular reaction is not a simple combination of two reflexes, but a combination in which one reaction is steered and regulated by the other one. A new aspect of the mechanism of consciousness takes shape: its regulatory role with respect to behaviour” (*ibid.*). Vygotsky's insertion of consciousness into the concept of the circular reaction seems not only to raise the concept to something approximating a feedback system, but indeed a conscious feedback system. And additionally he introduces the crucial question of its role in the regulation of behaviour, and the search for the mechanisms of such regulation. Luria's first major Moscow experiment had involved the regulation of behaviour and the characteristics of its breakdown. Vygotsky's new formulations must have made an enormous impact on Luria. If, ultimately, psychoanalysis failed both to connect with objective psychology, and the instincts of Freud's work did not

substantially progress beyond the status of ‘myth’, Luria had a whole new complementary, and closely related field available, crying out for someone like him to research it.

In a summary that would have pleased Kornilov, Vygotsky added, “consciousness may [thus] be ... reduced to the transmitting mechanisms of reflexes operating according to general laws, i.e., no processes other than reactions can be said to exist within the organism” (73). Therefore non-manifest reflexes (tacit speech), internal reflexes which are not accessible to direct observation by the observer, can often be exposed indirectly, in a mediated way, *via* the reflexes that are accessible to observation, and for which they form the stimuli” (ibid.). And in conformity with Luria’s practice, word association and psychoanalytic methods were acceptable for this purpose.

Conceiving of consciousness as an intrinsic part of the nervous system had another methodological advantage for Vygotsky. This origin of consciousness lies in experience “and thus the fact that it is conditioned by the environment. Being determines consciousness – for the first time, and after some elaboration, this law can acquire precise psychological meaning and reveal the very mechanism of its determination” (76). Here Vygotsky introduces another major concept of Herder and Marx, that being determines consciousness. But as the penultimate section shows, although it can be an individual consciousness, it is also a social consciousness. This section repeats many of the points of the earlier talk – about reversible reflexes (words), ‘reflexes of social contact’. And how “the mechanism of knowledge of the self (self-consciousness) and knowledge of others is the same” (77). Vygotsky had successfully, though not consciously, resurrected key aspects of the linguistic philosophy of Herder within the framework of psychology, and in the context of recent research. It was an extremely plausible position, which promised a broader and consistent perspective for psychology. Yet Vygotsky and Luria were not to start investigating in depth the relationships involving language and these mechanisms until the late twenties.

Chapter 4

Biology and Metaphysics, Physiology and the Promise of a Scientific Psychology, and Affect in Murder Suspects.

I

Freudian Metaphysics and the Search for Alternative Approaches

Vygotsky's interest in psychoanalysis was apparently of a more limited scope than Luria's. He referred to Freud in his posthumously published work, *The Psychology of Art*. In connection with this he was invited, presumably by Luria, to speak as a guest at the Psychoanalytic Society on "The Application of the Psychoanalytic Method in Literature" on 4 December 1924. Luria summarized its contents, "In certain cases aesthetic stimulus may produce 'pain' as well as aesthetic pleasure. Thus every poetic creation is ambivalent in character: its form is that which renders perception not easier, but more difficult and induces a transformation effect" (Luria 1925c, 244). In his article on consciousness Vygotsky discussed the social nature of language. "The individual aspect is constructed as a derived and secondary aspect on the basis of the social aspect and exactly according to its model. Hence the dual nature of consciousness: the notion of the double is the picture of consciousness that comes closest to reality. It comes close to the differentiation into ego and id that Freud disclosed analytically. He [Freud 1984c, 364] says that 'in its relation to the id, [the ego] is like a man on horseback'", i.e., not always in control. (Vygotsky 1997c, 77). Although Vygotsky uses Freud's metaphor for his own purposes, his familiarity with it, and preparedness to use it, display a close interest in Freud's writings.

This is evident in a preface by Vygotsky and Luria, which preceded Moshe Wulff's introduction to the first Russian edition of Freud's *Beyond the Pleasure Principle*. It may be that Vygotsky and Luria contributed different sections of it, though it is rather strange that Vygotsky subsequently wrote of it in 1926 (Vygotsky 1997b, 265) using the phrase "in the preface that I wrote". This may be of little consequence, but as we shall see, the differing evaluations of psychoanalysis and subsequently different criticisms of it by Luria and Vygotsky rumbled on until after 1930. Furthermore, while Vygotsky's initial reactions to

psychoanalysis are often presented today as sounder than Luria's, some of Vygotsky's remarks suggest that those elements of naivety that Luria himself admitted to, were complemented by those of Vygotsky.

In the preface they write, "in Russia particularly, Freudian psychology is very popular, not only in learned circles, but also among the general reading public. During the past few years almost all Freud's works have been translated into Russian and published. In front of our eyes, a new and original psychoanalysis is beginning to form in Russia, which, with the theory of conditional reflexes, attempts to synthesize Freudian psychology and Marxism and to develop a system of reflexological Freudian psychology in the spirit of dialectical materialism. Such a translation of Freud into Pavlov's language is an attempt to decode the dark 'depth psychology', and it is living proof of the great vitality of this theory and its inexhaustible research potential" (Vygotsky and Luria 1994a, 10-1). As regards the work being introduced, even before its translation, "a lively discussion of the problems raised in it began in Russian scholarly circles" (15) – as we saw from the comments arising in Trotsky's letter to Pavlov. "Even orthodox psychoanalysts sometimes pass over this work in silence..." (11).

The first half of *Beyond the Pleasure Principle* is, for Freud, relatively uncontentious, and Vygotsky and Luria pass this by. It is the part that deals with Freud's overt speculations about the life instincts and the death instincts that concerns them. They consider Freud's previous work to have been revolutionary and intrepid, but the present work "contradicts some fundamental ideas which had earlier been put forward by Freud himself" (11). Some would, however, differ (cf. the commentary in Freud 1984d). Freud posed the life instincts as disruptive, creating tension by means of excessive amounts of stimuli. In contrast, the pleasure principle sought relief from this. Having examined the repetitive behaviour of some infants, Freud discussed the role of repetition in life. He concluded that we unconsciously sought to repeat earlier stages – not simply of our life, but the evolutionary stages of life itself. Indeed it was the reversion to a 'primitive' survival system that W.H.R. Rivers suggested in order to explain the 'war neuroses'. This system bypassed conscious processes and therefore could not be attributed to the Freudian concept of the superego's censorship (Rivers 1922).

[Joseph Ledoux (1998) has recently proposed just such a separate pathway for memory via the amygdala]. Freud, however, barely mentions the ‘war neuroses’. Nor, incidentally, does he refer either to Ernst Haeckel’s formulation that ‘ontogeny recapitulates phylogeny’, Sabina Spielrein’s earlier work on the death instinct, or his own interpretation of Hughlings Jackson’s ideas of neurological evolution and dissolution. In itself this is, of course, no reason to discount their influence, and we shall look later at Haeckel (cf. V, i, 119, & iv, 146-7) and Jackson (cf. V, iv, 146-7; VII, ii, 199-204). Instead, however, he refers to Ewald Hering, Schopenhauer, and the principle of Nirvana. In regard to instincts, he writes, “our views have from the very first been *dualistic*, and today they are even more definitely dualistic than before —now that we describe the opposition as being, not between ego-instincts and sexual instincts but between life instincts and death instincts” (Freud 1984d, 326).

The pleasure principle of the title is subsumed within the death instinct. As Vygotsky and Luria put it, “even more elementary than this [pleasure] principle, and however paradoxical it may sound, is the principle of the death instinct, which is a basic, primordial and universal principle common to all living matter” (Vygotsky and Luria 1994a, 12). Not surprisingly they note (*ibid.*) that “one may easily get the impression that in this case we are dealing with metaphysical speculation rather than scientifically reliable propositions”. Perhaps Freud is “attempting to smuggle in the decadent philosophy of Nirvana and death under the guise of biological principles” (13). Although the work is speculation, it is, nevertheless, scientific speculation. “It is metapsychology, not metaphysics. This work is a step beyond the boundaries of empirical knowledge, but not into the realm of the transcendental and supersensory, only into the domain of the hitherto insufficiently explored and unilluminated... [Freud] would be only too happy to exchange the metaphorical language of psychology for physical and chemical terminology...” (*ibid.*).

Once and for all [Freud’s hypothesis] breaks completely with any teleological concepts in the spheres of psychology and biology. Every instinct is causally dependent on its previous condition, which it strives to reinstate. Every instinct has a conservative character and is impelled backwards and not forwards. And this is

how a bridge (a hypothetical one) is thrown across the science of the origins and development of organic life to that dealing with inorganic matter. Thus, in this hypothesis, for the first time, the organic whole is decisively integrated into the general framework of the world (14-5). At the root of all the proposals in this book lies one single tendency, namely an attempt to produce a general biological scheme for psychic life (16).

The concluding paragraphs are remarkable. Vygotsky and Luria ask that, if the above position holds, “how can humanity’s development from lower to higher forms be explained?”

[Freud suggests that the answer lies] in the external conditions of the material environment in which the individual exists. It is they that represent the true basis of progress, it is they that create the real personality and make it adapt and work out new forms of psychic life; finally they are the ones that suppress and transfer the vestiges of the old conservative biology. In this respect Freud’s psychology is thoroughly sociological and it is up to other psychologists who find themselves in better circumstances than Freud to reveal and validate the subject of the materialist foundations of this theory (ibid.).

According to Freud, the history of the human psyche embodies two tendencies, the conservative-biological and the progressive-sociological. It is from these factors that the whole *dialectic of the organism* is composed and they are responsible for the distinctive ‘spiral’ development of the human being. This book represents a step forwards and not backwards along the path to the construction of a whole, monistic system, and after having read this book a dialectician cannot fail to perceive its enormous potential for a monistic understanding of the world” (16-7).

[Thus] bourgeois science is giving birth to materialism... (17).

Vygotsky and Luria certainly place a more optimistic, ‘progressive’ gloss on Freud’s views than Freud did himself. Given the ‘mythological’ nature of Freud’s speculations, it seems reckless to attempt to build a materialist science on such speculative foundations. Luria’s own account in *Psychoanalysis as a System of Monistic Psychology* is, by comparison, relatively cautious. There he talks of internal and external stimuli. This account seems, by contrast, to be far too adventurous for him. The emphasis on the contradiction between conservative-

biological and progressive-sociological, together with the talk of the ‘spiral’ nature of development suggest that it was more likely to have been a formulation of Vygotsky that employed Hegelian logic. Luria’s earlier approach might be considered eclectic by some, but, like reflexology, its methodology relies on stimuli and reactions. It could therefore be argued that it was consistent - even if it thereby initially failed to account for higher order systemic features. In contrast, the joint version outlined above relies on the combination of two contradictory systems, of which one is purely speculative. If one was to diagnose one version as being the more eclectic, their joint version is the clear favourite. Although it might *seem* to offer a major advance for Luria, by changing the emphasis from stimuli to dynamic Hegelian systems, these were notional, unlike the ones he was working on using the combined motor method.

Therefore it is surprising that in the *Historical Meaning of the Crisis in Psychology*, written in 1926, Vygotsky not only strongly criticizes Freud, but also differentiates his own views from Luria’s, which he describes as eclectic, yet, at the same time, he continues to defend *Beyond the Pleasure Principle*. His defence of it remains firm, yet his memory of how he actually introduced it is highly selective and, for this reader, inaccurate. “In the preface which I wrote for the translation of Freud’s book ... I attempted to show that the imaginary construct of a death drive, despite the whole speculative nature of this thesis, ... satisfies the need of modern biology to master the idea of death... Indeed, death is a universal law of living matter”. It cannot be treated as if it made no sense or simply in an abstract negative sense – a view endorsed by both Engels and Hegel (Vygotsky 1997b, 265-6).

It was precisely this idea that I defended in the ... preface..., the need of biology to master the concept of death from a fundamental viewpoint... Despite this, I did not declare Freud’s solution to this equation to be a highway in science or a road for all of us, but an Alpine mountain track above the precipice for those free of vertigo. I stated that science needs such books as well: they do not reveal the truth, but teach us the search for truth, though they have not yet found it. I also resolutely said that the importance of this book does not depend upon the factual confirmation of its reliability: in principle it asks the right question (266).

The disparity between the positions adopted in the preface and his defence of it represents a major shift. Namely, an attempt to defend the book on biological as opposed to psychological grounds, and an attempt to defend it as speculation concerning a specific issue, rather than as a guide to psychological methodology. Vygotsky's defence is quite unusual, and his use of Engels is misleading. In the *Dialectics of Nature*, first published in Russia in 1925, Engels writes of death in strictly orthodox scientific terms – i.e., cell death or chemistry (Engels 1987, 495, 572). A more charitable explanation of Vygotsky's comments is given by Iaroshevskii. Vygotsky suffered recurrent bouts of tuberculosis, one of which finally claimed his life in 1934. The 1926 bout was extremely severe, though he wrote *The Historical Meaning of the Crisis in Psychology* while in hospital. Luria told Iaroshevskii "that the doctors believed [Vygotsky] was a terminal case. Vygotsky himself whispered that he only had a few months to live". Hence, for him, "the problem raised by Freud was not just an abstract theoretical issue, but also an intimate and personal one" (Yaroshevsky 1989, 170-1).

Vygotsky's preface to an edition of Lazurskii (1925) casts an interesting, if confusing, light on both the preface to Freud and the later comments. It concerns Pavlov's concept of the conditional reflex, which he considered "could also be called connective" (Asratyan 1953, 92). "Due to this mechanism the organism can establish infinitely varied connections and relationships with the environment" (Vygotsky 1997d, 58). Its importance is crucial.

The mechanism of the conditional reflex is a bridge thrown from the biological laws of the formation of hereditary adaptations established by Darwin to the sociological laws established by Marx. This very mechanism may explain and show how man's hereditary behaviour, which forms the general biological acquisition of the whole animal species, turns into man's social behaviour, which emerges on the basis of man's hereditary behaviour under the influence of the social environment. Only this theory allows us to give a firm biosocial footing to the theory of the behaviour of man and to study it as a biosocial fact. In a sense, Academician Pavlov is quite right in saying that this theory must form the foundation of psychology: psychology must begin with it (ibid. 59).

Vygotsky continued to reiterate the sentiments of this last sentence for years (cf. Yaroshevsky 1989, 112, 212-3). Vygotsky's statement could equally well have been written by Luria. It also omits all mention of psychoanalysis, as though, in this context, it were an optional extra to be slotted into the discussion of hereditary behaviour according to taste. On the other hand Vygotsky's emphasis on the role of biology in psychology could explain why in 1926 he saw Freud's contribution in terms of biology. His concern with seeking 'bridges' is here transferred from that of moving from inorganic to organic matter to that of moving from biology to sociology.

We know that Vygotsky supported V.A. Vagner's criticism of Pavlov's attempt to explain all instincts in terms of reflexes, but Vagner (1849-1934) was better known for his views concerning evolutionary development. Although there was a 'pure' line of instinctual development, Vagner also proposed another line of 'combined' development whereby the 'intellect' develops on the previous structures. "The relationships between 'instinct' and 'intellect' were viewed by Vagner as those of gradual subordination of the former by the 'intellect' in the course of phylogenesis" (Van der Veer and Valsiner 1991, 195). Although all appear to agree on the influence of this theory on Vygotsky, its influence is only attributed to the late 1920s. Clear evidence is lacking for this earlier period. Vygotsky was almost certainly weighing such things in his mind.

In the preface to Lazurskii he continues, "only a scientific system which discloses the biological meaning of mind in behaviour will point out *exactly which new element* [my emphasis-MH] adds to the organism's reaction and will explain mind as a behavioural fact. Only such a scientific system can claim the title of a scientific psychology. Such a system has, however, not yet been created" (Vygotsky 1997d, 60-1). He adds:

It will come as a broad biosocial synthesis of the theory of animal behaviour and societal man. This new psychology will be a branch of general biology and at the same time the basis of all sociological sciences. It will be the knot that ties the science of nature and the science of man together. It will therefore be most

intimately connected with philosophy, but with a truly scientific philosophy which represents the combined theory of scientific knowledge and not the speculative philosophy that preceded scientific generalizations (ibid. 61).

This was an approach that was consistently developed after 1926. Here Vygotsky is making a much more realistic suggestion than in either the preface to Freud or the subsequent comments, but - paradoxically and just like Luria - simultaneously with the views he expressed in favour of Freud.

In a later article in the *Great Soviet Encyclopaedia*, Luria and I. D. Sapir (1930, 613) criticized the “metaphysical characteristics” of the life and death instincts, and Freud’s assumption that all other instincts derived from them. “It is easy to see how far these attempts depart from scientific psychological approaches to the question of drives, here presented as primordially given very primitive processes of an almost cosmic order. The classification itself serves as an attempt to express in psychological terms the processes of the disintegration and restoration of living matter”. Luria and Sapir’s article was actually printed in 1928, but held up while a key political article on the All-Union Communist Party was revised (Scheerer 1984, 322). But Luria had already used another encyclopaedia article to criticize other instances of a metaphysical approach in psychoanalysis, namely, the ‘psychologistic’ explanations and Nietzschean philosophy to be found in the work of Adler (Luria 1926b). This short entry clearly confirms that the exploration of the impact of external social stimuli, whose investigation in the work of Adler had been promised by Luria in 1925, had also come to nought. Luria still occasionally referred to other aspects of Adler’s work. Vygotsky, although critical of Adler, went further, finding his conception of ‘compensation’ to be of interest for his work with what were termed “abnormal children” (cf. Van der Veer & Valsiner 1991, 65ff.).

This period in the work of Luria and, more surprisingly, Vygotsky might leave one confused. Superficially it appears as though their work was taking them willy-nilly in the right direction, but they were continually being diverted, but this is not really so. It is more to do with our expectations – or perhaps just this reader’s expectations - and here Vygotsky’s case is very instructive. He is probably best

known for his book, *Thinking and Speech* (1934). His talks in 1924 discuss language and consciousness, notably, in reference to Pavlov, as a system of inter and intra-personal reflexes. They seem like a natural preamble to his book. Similarly his comments above on conditional reflexes and the “extra element” seem to be leading inevitably in this direction. In one well-known passage he cites Marx’s remark that “the anatomy of man is the key to the anatomy of the ape”, which he explains. “A certain stage of development and the process itself can only be understood when we know the endpoint of the process, the direction it took, and the form into which the process developed... Having arrived at the end of the path, we can understand more easily the path in its entirety, as well as the significance of its different stages” (Vygotsky 1997b, 235). Yet in this 1926 work he did not develop this insight. In hindsight it could be said that, as a Marxist, Vygotsky did not yet fully grasp where some of Marx’s ideas came from – especially concerning human nature, language, consciousness, and the role of language in mediating consciousness. Marx, as an heir of the Enlightenment and of Herder, thought that humans were potentially capable of organizing their lives rationally. Like Herder, he did not think that humans were innately rational, but that rationality could be developed as humans gradually and scientifically understood their relationships to nature, their own history and the distortions introduced by various forms of the economic organization of societies. In the 1920s, however, psychologists still tended to see materialism as related exclusively to physiology and biology. As Vygotsky wrote in 1926, “the new psychology proceeds from instincts and drives as the basic core of the mind...” (Vygotsky 1997d. 61).

Thus an attempt to apply Vygotsky’s interpretation of Marx’s dictum to his *own* work does not yet illuminate all the stages of Vygotsky’s - or Luria’s development. They thought that working from the bottom up was a necessary and unavoidable phase in the theoretical development of psychology, and that eventually they would arrive at the area of higher mental functions. The alternative, working from the top down, especially in relation to language and consciousness, almost inevitably leads to idealist distortions, displayed historically in an enormous variety. Luria and Vygotsky, like their contemporaries, mined the seams of physiology, psychoanalysis, and empiricist research in pursuit of a

materialist psychology. And although they recognized the one-sidedness of these approaches, they also considered them certainly understandable and often legitimate. Vygotsky's *Historical Meaning of the Crisis in Psychology*, a book more praised than read, makes many interesting and relevant methodological points about transforming psychology into a unified science. He, however, never claimed it to be a blueprint for psychologists; Vygotsky knew the direction, but not the solution – at least not this side of the socialist transformation of society (cf. Vygotsky 1997b, 332, 342). With regard to language, it might seem that his early references to Potebnia and Potebnia's use of Humboldt were not necessarily evidence of an interest in the application of language theory to psychological development, but stemmed more from his literary background. Similarly his references to language in respect of reflexes might be seen as simply a logical corollary to Pavlov's theory – a commentary rather than a theoretical development with language as a key. That this was not the case is evident from his remarks on the inter- and intra-personal role of language. It has also been claimed that Potebnia's ideas played a crucial role in Vygotsky's work in the late 1920s (Kerecuk 2001, personal communication). On the other hand, it was also explicit in Pavlov's distinction of unconditional and conditional reflexes that the latter could be open to voluntary control. Hence it was possible to use these sciences without reducing psychology simply to an epiphenomenon of them.

Pavlov argued (1928, 113) that “the simple, the elementary is always conceivable without the complex, whereas the complex cannot be conceived without the elementary”. Vygotsky (1997b, 236) criticized this position, as did Luria (1926c). The position that ‘the whole is greater than the sum of its parts’ is almost inescapable with regard to human psychology, and, as we shall see in the following chapter, Vygotsky and Luria's appreciation of this was enhanced by their reading of the recent *Gestalt* school of psychology's focus on this very approach. Thus, although Vygotsky and Luria continued to use Pavlov and, to a lesser extent, psychoanalysis, they were well aware how reductionist they could be, and also that there were several countervailing positions that gave scope for development. On the other hand, neither Vygotsky nor Luria found the alternatives totally satisfactory.

Several other issues would be illuminated in the light of such an interpretation of Vygotsky's development. It would explain why Vygotsky's creative period began several years after his arrival in Moscow. And why the most significant phase of Vygotsky and Luria's collaboration was delayed until this period. In the earlier phase Luria's difficulties and admissions of youthful naivety were not dwarfed by Vygotsky's magisterial solutions, but mirrored in Vygotsky's own struggles to create a scientific psychology. But this is not the whole story in respect of the role that language played in Vygotsky's thinking in the period around 1926. Although the above interpretation seems to be a distinct option, Vygotsky, as we shall see, did introduce language in a key role, but one with a seemingly less high profile.

Luria was to retain his active ties to the psychoanalytic movement until 1927. Both he and Vygotsky retained an interest in its less fanciful ideas. But my reading of Vygotsky's attempt to justify the preface, and Luria's rejection of psychoanalytic metaphysics is that both felt they had gone too far in accepting Freud's metapsychological pretensions. Making sense of human psychology requires theorizing, and elements of psychoanalytic thought had proved to be a catalyst to Luria. Freud's metapsychology, on the other hand, had proved to be a diversion. Luria's recognition of this probably explains why he never wrote the book of which *Psychoanalysis as a System of Monistic Psychology* was intended to be a part. Luria had intended to publish an article on *The Experience of Objective Psychoanalysis* (cf. Kozulin 1984, 89) in a work to be edited by Kornilov, which did not appear (cf. V, iv, 141-2; VI, iii, 170ff), and he also referred to another unpublished article, *The Dynamic Moment in Psychoanalysis* (mentioned in Luria 1926a, 41). Both these probably refer to the experimental work he began in 1925, and hence did appear later within an amended format. When in the *Nature of Human Conflicts*, Luria (1932a, 150-1) does again refer to "experimental psychoanalysis", the quotation marks suggest to this reader that it is intended more in the sense of an analogy, or even in self-deprecation, though, of course, the ambivalence is unmissable. We will start to examine this in the third section of this chapter. In section two we will look at how Luria tried to show the significance of Russian physiology for the psychoanalytical movement.

Fortunately Vygotsky and Luria's previous support of Freud's metapsychology did not noticeably affect their practical work, other than in their rationalizations of it. And even here it is possible to argue that they also possessed parallel, and much more realistic, rationalizations. These were not only showing their effects in their practical work, but also developing a momentum of their own. By 1926 Vygotsky appears to have found the "new element" that extended conditional reflexes. In another preface, this time to Thorndike, he writes, "new links may be established ... by combining a conditional reflex with a new stimulus. In other words, new links may be formed and develop not only on the basis of innate, but also on the basis of conditional reflexes". Conditional reflexes of the second degree and beyond – super-reflexes – "allowed man in particular to develop all complex forms of mental activity and work activity" (Vygotsky 1997e, 156). "In short, man's behaviour is revealed in all its real complexity, its enormous significance, as the dynamic and dialectical process of a struggle between man and the world and within man. This is the first basic idea of the new psychology" (ibid. 157).

Eventually Pavlov too extended his scope beyond conditional reflexes of the second order, and reflexes as 'signals' (cf. Pavlov 1928, 123, 382) to what he called the second signal (or signalling) system (Pavlov 1941, 93, 113-4, 179, 181). For a scientist in his eighties – the works were written in 1932 and 1935 – it was a major step for Pavlov to formally extend reflexes to speech.

The first system of signalling reality is the same in our case as in the case of animals. But words have built up a second signalling system, which is peculiar only to us, being a signal of the primary signal. The numerous stimulations by word have, on the one hand, removed us from reality, a fact we should constantly remember so as not to misinterpret our attitude towards reality. On the other hand, it was nothing other than words which made us human, but this, of course, cannot be discussed here in greater detail (ibid. 179).

Unfortunately Pavlov did not live to elaborate or make concrete the implications. He had long been overtaken by the younger generation, but, as we have seen, these felt there were other problems in Pavlov's concept of the reflex. In conversation with Michael Cole, Luria clarified Vygotsky's views. "It was

Vygotsky who provided the theoretical solution to the problem [of the higher forms of mental activity]. He accepted the idea that even the most complex psychological processes are based on the combination of elementary reflexes, but he felt that attempts to reduce mental activity to a system of reflexes was the wrong way to proceed" (Luria, cited in Cole & Cole 1971, 82; cf. Yaroshevsky 1989, 112-3). Here, then, we have a kind of confirmation that Vygotsky's 1924 lectures were not a direct form of logical preamble to his later work, nor, implicitly, did he yet have the solution in 1926. On the other hand, Luria tells us of another area of investigation involving Vygotsky and language. "Vygotsky and I first tried to find out what brain mechanisms were the basis for the *regulating* role of speech in 1926 at the Clinic for Nervous Diseases" (Luria, in Cole & Cole 1971, 88). Although their initial assumptions were apparently proven to be wrong, the role of speech in the regulation of behaviour was to become one of their major concerns - indeed a concern that was intimately bound up with Luria's life work.

II

A Review of Russian Physiology

Modern Russian Physiology and Psychoanalysis, to which Luria referred in his 1925 report on the Moscow debates (see the end of chapter two), was finally published in the psychoanalytic press in early 1926. In it he recapitulates, amplifies and extends his earlier comments, notably on the relevance of Ukhtomskii's artificial creation of a dominant reflex for the psychoanalytic concept of drive, together with the work of Pavlov and his school. He also looks at the work of N. E. Vvedenskii. We will look first at his comments on Ukhtomskii and Vvedenskii, which are interesting in their own right. Then we will look at the areas of Pavlov's work that are new to Luria's published work. It was probably as a result of two works published in 1925, one by Pavlov and one by his colleagues, that Luria was stimulated into writing the article that is the subject of this section. All these areas offer tantalizing glimpses of the way Luria was thinking about neurology, and point to the future development of his ideas. Although remaining on the surface deferential to Freud, it too points the way to "the new psychology".

Luria begins by stating that a number of psychoanalytical concepts had been endorsed by the findings of experimental biological research. He again specifically mentions that concerning “internal secretions” (hormones). “In the latest works on the physiology of nerves we also see a clear endorsement of psychoanalytic positions” (Luria 1926a, 40). Specifically, Luria addresses a major topic, which seems to bypass the solution both of *Psychoanalysis as a System of Monistic Psychology* and the preface to Freud’s *Beyond the Pleasure Principle* by Vygotsky and himself – i.e., the contrasting of internal and external stimuli, of drives and social influences. Namely, “the fundamentally deeper relationship of internal and external stimuli, treated by Freud in *Beyond the Pleasure Principle*, in which he voiced the opinion that the drive (the inner impulse) is, in the final analysis, only the deposit [*Niederschlag*] of earlier impulses, which arose from the outside world” (49-50). The findings of Russian physiologists were here particularly significant.

A.A. Ukhtomskii’s theory of the dominant began with a “study of the physiology of the sexual drive of the frog”. This concluded that:

A special centre of arousal exists in the nervous system which takes over incoming stimuli to the organism and dominates the residual reflexes. This centre was called the ‘dominant’ by Ukhtomskii, for demonstrating its continuity and persistence on behalf of the dominant arousal... Its basic qualities correspond fully to the definition of drive as a continuous inner stimulus that Freud gave. ... During the action of the dominant other reflexes become inhibited – that means that in the presence of a permanent arousal (a drive) all the activities which have no relation to it become weakened or cease. Stimuli directed at other reflexes are, as it were, taken over by the dominant processes and thereby strengthen them (50). [Luria added:] These laws are well known to us from psychoanalytic practice (51).

The well-known stereotypical behaviours of frogs are not necessarily a sound basis on which to base any form of human psychological analysis, and Vygotsky’s previous criticism of this is more than justified in the light of our well-known differences from frogs. But Luria was concerned here with more fundamental issues, namely the problem of whether or not it would be possible “to *artificially produce* [my emphasis-MH] such a centre of permanent arousal that obeyed the

laws of the dominant. Freud ...conjectured that the drive itself was in principle not to be distinguished at all from manifest stimuli, and that at the beginning of organic development the drives were perhaps only the condensing of manifest stimuli within the cell" (ibid.).

Ukhtomskii's research showed that it was indeed possible to produce "a 'deposit' of manifest stimuli in the muscular-nervous apparatus". By intensive stimulation he created a dominant reflex in a muscle. Thus "the excitation in muscle B was taken over by the dominant reflex of muscle A. In the muscle-nerve preparation, therefore, ... an elementary model of drives was created, and thereby the connection was demonstrated in principle between a manifest and a permanent internal stimulus, the drive. Freud's conjecture thereby obtained its experimental confirmation, if only in a preliminary and elementary form" (ibid.). At this stage Luria linked the development of drives to the history of the organism's relationship and adaptation to the environment. This is an approach that is far from being the exclusive property of Freud. Indeed, Pavlov, in a similar fashion, proposed that conditional reflexes could be transformed through inheritance into unconditional reflexes (cf. Pavlov, cited in Asratyan 1953, 96). It bypasses both the eclecticism of which Vygotsky accused him, and also the eclecticism in which Vygotsky partook. This was a model that at some level could also be related to the acquisition or organization of higher mental functions, together with Vygotsky's propositions concerning conditional reflexes of higher degrees. On the other hand, Vygotsky appears to have supported V.A. Vagner's opposition to this argument.

The physiologist N. E. Vvedenskii (1852-1922) (not to be confused with the philosopher A. N. Vvedenskii) was also significant for psychoanalysis, said Luria, for "confirming experimentally that inhibition is only a result, and a special form, of over-stimulation" (Luria 1925a, 48). "Vvedenskii characterized this inhibition as 'parabiotic'. He considered it to be not only an inevitable consequence of every corresponding process of stimulation, but also to be a biologically useful mechanism. One which supports the organism in its efforts in preparing to cope with too great a number of stimuli, which could become dangerous to its life or its orderly functioning" (48-9). It was thus consistent with the operation of the pleasure principle and the reduction of tension. "From the standpoint of

Vvedenskii's physiology, the mechanism of repression appears as a special case of parabiogenic inhibition. The feeling of aversion which can be traced back to overstimulation thus leads to an excessive stimulation of the cortex, whose consequence must be a 'parabiogenic inhibition' of the cortex, and consciousness" (49). Pavlov also saw stimulation and inhibition "as different sides, different manifestations, of one and the same process", which can nevertheless be transformed into one another, depending on whether the body needs to preserve energy (cited in Asratyan 1953, 110, cf. also 118-9).

Although the whole section connected to Pavlov's work is significant, especially as regards the types of experimental research Luria was undertaking, the first part crystallizes some of the new ideas crucial for the development of Luria's thought. Kurt Goldstein considered Luria's comparisons of the approaches to nervous processes of Pavlov and psychoanalysis to be "unusually interesting and his conclusions careful" (Goldstein 1939, 308). Luria sets the scene:

Classical physiology could not comprehend the possibility of 'psychogenesis', mainly on the extremely primitive basis that it proposed. In the final analysis all human activity was to be completely dissected into a number of constant, unchanging, and strictly localized functions, which could be traced back in turn to the activity of quite specific 'brain centres'. It would uncover the centre for speech, for writing, for 'notions', and so forth, so that one gets the impression of a system quite close to the phrenological views of Gall (Luria 1926a, 40-1).

This was the physiology associated with 'faculty psychology' and 'associationism'.

These views were made redundant in psychology by dynamic ways of thought, which conceived the psyche and human behaviour as a moving equilibrium, which changed continuously under the influence of internal impulses and the influences of the outside world. Psychoanalysis placed this position most clearly at the forefront; other psychological systems followed it, such as behaviourism and the new Gestalt psychology (41).

At this point Luria refers to his 1923 book, *Psychoanalysis in the Light of the Basic Tendencies in Modern Psychology*, a work he evidently considered still to be of value. He also referred to his article, *The Dynamic Moment in Psychoanalysis*, for which he optimistically gave the publishing date as 1925. It was not published. He continues, "A similar process is indicated for the development of modern physiology. By this we mean above all the alteration of concepts of 'brain centres' and the working of the cerebral cortex, and equally the change of the physiological outlook especially regarding the above-mentioned question of psychogenesis" (ibid.). This occurred in the work of I.P. Pavlov, whose investigations of the secretion of digestive juices in dogs accidentally came across areas of psychogenic events. The conditional reflexes, where a reflex was prompted indirectly by a signal, e.g. a bell presaging food for the dogs, led to salivation. That is, the bell induced a psychic act, which in turn brought on the salivation.

Thus the organism presents a system that is connected with the variety of the outside world by a free mechanism, a system of moving equilibrium, which possesses the facility of complex adjustment. From this viewpoint it is understandable why the organism always possesses the possibility of responding in very different ways to new circumstances in the environment. Thus the problem of 'psychogenics' receives a physiological basis (42).

Nevertheless, Luria recognized that these statements required further experimental data and supplementary hypotheses.

I.P. Pavlov proposed the notion that, alongside the stable, long-standing brain centres, were dynamic, temporary, 'conditional' centres that, as it were, build themselves anew every time - whenever the perceptual field encountered new stimuli. These 'arousal centres' are joined to other stronger centres which draw over to themselves the energy of almost every specific temporary centre and will impose on them its own reaction, so that in these cases they also react to a 'conditional reflex' (42-3. Relevant passages for the last three paragraphs include Pavlov 1928, 324, 330, 332).

The problem of the function of these ‘temporary centres’, and thus also that of psychogenesis, thereby becomes one of the basic problems of physiological mechanism and hence obtains not only its thorough foundation, but also its place in the general lawfulness of organic events. It becomes comprehensible as a case of the connection of several nervous centres, through which a complex perception leads to a change of activity of the centres entrusted with organic functions (43).

Luria is here talking about the relevant chapters of the third Russian edition (1925) of Pavlov’s work, *Twenty Years’ Experience of the Objective Study of Higher Nervous Activity in Animals* (cf. Pavlov 1928 chapters 33, 35, & 36). Although it relates to psychogenesis, it refers to the adaptation of the animal to its environment at a relatively simple level compared to some operations performed in a human’s higher mental functions. Nevertheless, it seems to me as if Pavlov supports a basic model of the operation of the brain that, by confirming Luria’s own researches, helped to confirm him against competing views of its operation. The latter were those of the old extreme localizationists and those who proposed overall activity. Although one shrinks from proposing Pavlov as an early precursor of the theory of neuroplasticity (!), it is even possible to see in Luria’s comments, perhaps for the first time, a germ of something like the concept later to be known as that of the ‘functional system’. It lacks the sophisticated ramifications of the concept that he applied to it after 1929. It is not as yet situated in a discussion about ontogenetic development: hence it does not deal with how such systems arise and develop in the higher mental functions. It has therefore not been visualized in terms of the varied environmental influences of culture and historically developed forms of cognitive mediation in those higher mental functions, but it is there at least in embryonic form – here somewhat ironically as a dinner bell! That said, I think we shall find that the term ‘functional system’ is implicit in Luria’s own combined verbal and motor method, and that Pavlov’s comments merely encouraged Luria, and give us the opportunity to see how his mind was working. One can assume that, with this probing mind, Luria was bound to develop it further, especially as his close friend Vygotsky was, as we have seen, thinking along similar lines. Vygotsky used the first (1923) edition of Pavlov’s work for his book, *Pedagogicheskaja Psikhologija* (1926), though apparently

more in relation to an extension of conditional reflex theory (cf. Yaroshevsky 1989, 71-4), rather than to move beyond it.

Pavlov's research extended beyond such simple forms of psychogenesis to the investigation of the consequences of apparent contradictions that could be induced within the psychogenic scheme. Notably Pavlov trained a dog to respond as part of a conditional reflex to a note of a particular pitch – but to no other pitch. “Physiologically speaking, that means he connected the function of a specific area of the brain to the activity of the pre-digestive system and, at the same time, inhibited other neighbouring areas” (Luria 1926a, 43). An animal “with a labile nervous system” was tested using a range of different pitches. It was easily able to distinguish the sounds in the simpler exercises – responding to, or ignoring, the sound.

But as soon as an excessively difficult exercise [in discrimination] was set for the animal, the correct discharge of the reflexes was interrupted, and eczema broke out on its skin. This eczema went when the dog was allowed to quieten down. Here we have a clear demonstration under experimental conditions of a psychogenic symptom elicited from a dog. [This involved] ... a flight from the difficult exercises set by life into a neurotic symptom, or by the conversion of an affect. The problem of psychogenesis receives a physiological foundation (43-4).

And, furthermore, one involving what psychoanalysis would describe as an affective mechanism. Later (cf. VII, i) we will discuss Luria's own study of ‘natural’ and experimentally introduced emotional conflicts into humans. There the same concept, termed by Luria the ‘conflict of setting’, led to a much greater development in understanding the processes involved, especially in relation to the operation of the higher mental processes in humans. Although freely admitting the influence of Pavlov and his students (Luria 1932a, 206-7, 209), Luria had the enormous advantage of being able to measure the various reactions of humans simultaneously, and thus the interactions of their verbal and motor processes.

“In the recent [*Works of the Laboratory of I.P. Pavlov* volume 1 (1925)], Pavlov's colleagues consider the conception of affect as a disruption of the normal

discharge of life-functions. They have graphically shown that its mechanism can become reduced completely to [a state of internal] conflict by an extremely difficult problem in life” (Luria 1925a, 44). Luria cites further evidence from the school’s experiments. The effect of a small amount of water seeping under a laboratory door, bringing back traumatic memories for the animal survivors of the Leningrad flood of 23 September 1924, induced similar reactions to those in the original event. Other experiments resulted in ‘nervous breakdowns’, e.g. “an abrupt change in behaviour: they curtailed their movements, became limp, and reacted weakly to stimuli”, depending on the animal being of an “inhibited type” (ibid. 46, cf. Pavlov 1928, 343-5). In other animals equally traumatic, but overt, reactions were induced. Luria concluded, “it is totally clear what significance an experimental confirmation of reaction types in physiology can have, together with assessment of constitutional types with respect to the field of psychic variations” (Luria 1926a, 46).

Finally Luria looked at the instigation of sexual inhibition, or rather, repression, induced in the Pavlov laboratory. It was found that stimulation of a particular area of skin in the hind leg of a normal dog produced a reaction that “showed all the signs of sexual attainment”. To remove this “inhibitory reactions were developed in neighbouring parts of the skin. A circle of inhibitory reactions gradually “drove in” until the sexual reactions were eliminated.

[As a result of] this artificial repression of the sexual reaction the character of the dog altered completely. Until then lively and active, the animal became sluggish, spineless, immobile and reacted only with difficulty to the its surroundings. This fact seems to be of extraordinary importance since, here in animals, a purely physiological experiment has brilliantly confirmed the thesis demonstrated by psychoanalysis of the overwhelming significance of the sexual apparatus for the whole psyche (47).

Kurt Goldstein later commented that it was “not quite clear whether Luria intends to explain facts discovered by psychoanalysis more on biological grounds or vice versa” (1939, 309). Thus we conclude Luria’s last published extended paper aimed specifically at a psychoanalytic audience. Although it is clear that Luria

continued to consider both Pavlov and Freud to have produced significant work, it was through his own experimental work and that of his colleagues, physiologists and others that he related primarily to both. Confirming ideas common to psychoanalysis and other approaches was to be his ‘contribution’. After all, as we have seen in the preface to Freud, analysts were not as well placed as psychologists to reveal and validate the material foundations of psychoanalytic theory! Seemingly the role of psychoanalysis as a catalyst to his thinking remained significant, but it was he who was to do the independent research, research that could also be accommodated within newer theoretical approaches. Psychoanalysis as such was a therapeutic approach, and its adherents probably provided Luria with little feedback. In both the short and the long run he was the one to develop new hypotheses and theories. He was to provide in a short time new methods of diagnosing functional neuroses, before going on to the diagnostic work for which he is most famous, in neuropsychology.

III

Affect in Murder Suspects

Fortunately, the ‘external stimuli’ of psychological ideas in the 1920s, together with Luria’s own ideas and ‘internal stimuli’, were sufficient to power his thirst for research throughout these years, and for the rest of his life. Among this research was a series of investigations that took place over five years into the question of affect in suspected criminals. Although this started in 1925 he did not publish his findings until he had completed a representative and varied sample of results. The published accounts are in Russian (Luria 1927a & 1928a), English (Luria 1932a – probably written 1928-9), and German (Luria 1930a), [the latter two are available to me]. “But that was only the tip of the iceberg”, said Luria in 1974. “A huge unpublished manuscript has been left over from those days. I recently turned it over to the Institute of Criminology, and it appears to have evoked some interest” (cited in Levitin 1982, 158). The destination of the manuscript is given, perhaps more precisely, as the Serbskii Institute of Forensic Examination (Homsкая 2001, 17).

“We proceeded on the assumption that a person who had, say, committed a murder and concealed the fact is sure to react affectively to the attributes of the murder. His overriding concern is not to betray himself and, naturally, all the words that remind him of the murder would lead to affective complexes which we could detect” with our results from the use of the combined verbal and motor method – as in the Moscow University experiment (Levitin 1982, 157). Initially, Luria set himself the task of deciding which two of a group of five people had been read a particular story. The two were soon revealed through the use of the combined method and a list of words connected with the story. After this “fanciful” test run (detailed in Luria 1930a, 154-61), Luria turned to the real thing. Although he refers to many of the previous experiments involving criminals, all such experiments had taken place after conviction (Luria 1932a, 79). So “I went to the Prosecutor’s office in Moscow, explained our idea, and soon a laboratory was organized there, and we were given an assistant, a young detective by the name of Lev Sheinin (who later became a well-known writer of detective stories). We were allowed to study the case and the suspects before they were questioned” (cited in Levitin 1982, 157). They were thus able to choose in advance words that would be relevant to the circumstances of the crime, though avoiding the actual deed, and place them on the list of word stimuli. This account is confirmed (Luria 1930a, 142), where he also makes it clear that the research was pure science and not used in the legal prosecutions (ibid. 162, note). In *The Nature of Human Conflicts* Luria elaborates, “we were in a position to perform our experiments on subjects who had been arrested a few hours before... When required by the experimental conditions, we obtained suspects before they were questioned and before they were told the cause of their arrest”. Sometimes, in order to uncover the structure of the process, experiments were repeated after the trial. Arrested suspects, subsequently found to be innocent, were among those who took part. Over five years, fifty subjects were examined. The majority of these “were murderers or suspected of murder” (Luria 1932a, 79-80).

Luria was dissatisfied with the earlier research with criminals, which had at one time been fashionable, but often did not use real criminals (ibid. 98). His later paper which is strictly concerned with the experiments, deals with his precursors in greater depth (Luria 1930a, 139-44). The American subtitle of Luria’s 1932

book was “Emotion, Conflict and Will”. These words characterize what gave his experimental designs the edge over previous work. Those suspects were examined before the verdict were not only already emotionally upset, but, if guilty, were placed in a situation of having to deliberately attempt to repress reactions to certain word stimuli. The similarities with Pavlov’s experiments are evident here, and he was quite open about them. Secondly, in contrast to the relatively passive examinations of earlier experimenters, the use of the combined verbal and motor response method involved the subject in both verbal and manual responses, i.e., active responses involving more neural networks that tended to betray the subject’s emotional responses even more through their interaction (Luria 1932a, 98).

The emotional trauma was considered to exhibit primary and secondary effects. The former related to and increased in relation to the circumstances and magnitude of the crime; the latter to the circumstances of the arrest and the expectations concerning the outcome – the punishment for serious crimes such as murder could be execution (77-78). Although the suspect was likely to be exceedingly upset, was it possible to separate out responses to the word stimuli which appeared to link him to the crime? And was there a difference between those experiencing primary effects alone and those with additional, or mainly, secondary effects? A survey of eight murderers and two controls is shown. The primary effects on the murderers were such that their reactions were disturbed throughout the experiment, almost regardless of the word stimuli. The same response was displayed by suspects who were later found to be innocent. As a consequence, in the version from 1928-9 (i.e., Luria 1932a, unlike Luria 1930a), Luria introduces research and concepts derived from other work of his that took place after the first criminal investigations. He compares the responses of the murderers with those of children of different ages. He writes, “in the child aged twelve and a half, the standard setting of language reactions is already established at a definite speed, and the behaviour at this stage is completely organized”. On the basis of these comparisons he concludes that the disturbed reactions and disintegrated co-ordination of the criminals resemble those of younger children who have not yet developed such skills (83). He later concludes (95) that the motor excitability demonstrated here “is evidence of the weakness of the

‘functional barrier’, of that difficulty in controlling excitation, and separates it off from the motor area, which is characteristic of a functional neurosis as well as of the actual affect”. Under these conditions Luria concluded that there was “no remarkable difference” between primary and secondary effects (96). In the following chapters we will begin to look at this new research and the new concepts that Luria was forced to employ.

Nevertheless there were cases where it was possible to measure reactions to individual stimuli as had occurred at Moscow University. In a sample of five cases using subjects who also included those subsequently found to be innocent, and those guilty of minor crimes, Luria achieved usable results. These enabled him to predict the actual criminals from other suspects. “We were able to use subsequent criminal evidence to verify our hypotheses” (Luria 1979a, 35).

Finally, Luria commented on the therapeutic effects of confession – consistent both with Christian and psychoanalytic practice. “Admission of guilt removes from the criminal those restraints which controlled each of his steps and every one of his thoughts and created an extremely acute conflict of very marked tension. Thus confession is a path to the relief of affect and to the re-establishment of a more normal functional life” (Luria 1932a, 115). But “we were least concerned with the criminological application of our work” (Luria, cited in Levitin 1982, 158). In part II we will look at the experimental situations that Luria considered more significant, though chapter 5 will be mainly devoted to the impact of the ideas of *Gestalt* theory on Luria.

PART II

THE NATURE OF HUMAN CONFLICTS

Chapter 5

The Influence of Gestalt Psychology

Introduction

Part II of this thesis is largely devoted to Luria's book, *The Nature of Human Conflicts*, published in America in 1932. In 1929 Luria attended the Ninth International Congress of Psychology in New Haven, Connecticut. He brought a copy of his work specifically to find a publisher. He placed it with the New York publisher Liveright. Afterwards he sent the final parts, containing his experimental work up until 1930, together with concluding and introductory chapters. W. Horsley Gantt, his translator, wrote, "I have made a close translation of the actual experimental work, without alterations or omissions. Owing to the large size of the book, however, the discussions I have sometimes condensed; Chapter XI and particularly Chapter XII [the last two chapters] I have abstracted quite freely without adhering to the style of the author" (Luria 1932a, viii). In addition to the excessively literal translation, which simply does not work in English, the book is difficult to digest, because there are so many ideas, implicit and explicit, contained within chapters devoted to experiments, together with more theoretical chapters. In contrast Henry Head's *Aphasia and Kindred Disorders of Speech* (1926) is divided into two volumes, the second of which deals with the individual cases, whereas the first deals with the general argument with illustrations from the cases. This model would seem to be appropriate for many approaches and theories, which revolve around individual casework. Although Luria's researches often hinged around the examination of individuals, and extended to diagnosis, much of the work involved many subjects, thus providing adequate samples for statistical analysis. Nevertheless, in retrospect, I feel Luria's work might have benefited from a different structure.

It may be that the forthcoming edition of the Russian original will provide the missing commentary that links the argument from chapter to chapter, and that Luria's conception is shown to be valid. Michael Cole (2002, personal communication) confirms that in the new edition there is a significant amount of

new material in the third part of the book. Barely 27 years old when he went to America, Luria's opportunism has certainly been justified historically. By the time of the book's American publication, Luria was under attack in Russia, and continued to be. If he had delayed further and reflected at length on its implications and presentation, it might never have been published anywhere. As it is, while the failure to publish the work in Russia under Stalin and his immediate successors is explicable, the continued failure to publish it within the last thirty years is something of a mystery.

In Part I of this thesis a generally chronological approach was adopted in order to show both Luria's development and the theoretical context in which it developed. I included Luria's experiments with Moscow University students and his research into affect in criminals and suspects within this framework. It is neither possible nor desirable to attempt a strictly chronological approach with the book as a whole. Part I showed many of the theoretical problems facing Luria (and Vygotsky) at that time, part II shows Luria's experimental response to them. Although the varied sequences of experiments took place over several years, few were published at the time, and, by and large, in *The Nature of Human Conflicts* they are all informed by the positions he took as he was writing up the book. Therefore Part II will look at the work and its theories as a whole, rather than in strict chronological order.

Despite the criticisms, we owe W. Horsley Gantt a great debt of gratitude for translating one of the masterpieces of psychology. We owe him a double debt for also translating key works of Pavlov (Pavlov 1928, 1941). A comparison of the works shows what an enormous revolution Luria introduced into psychology. Although at times directly inspired by Pavlov's experiments, and also initially by psychoanalytic questions, the book marks a decisive step beyond both Pavlov and Freud, and is explicitly critical of them. This is not disputed in the case of Pavlov. In the case of psychoanalysis there are several respected scholars who think otherwise. I attribute this largely to misunderstandings on their part. Wishful thinking plays a part in the views of others. Therefore my response involves discussing the sources of misunderstanding. A brief postscript to this chapter (section iv) will look at Pavlov and Freud. But firstly it makes sense to examine

the major role of the *Gestalt* school of psychology in moving the discussion forward from where we left it at the end of chapter four, and Luria's responses to these new ideas. In many ways this discussion, in itself, shows how Luria moved on from Pavlov and Freud. The first two sections of this chapter examine articles written by Luria on the approaches of the Gestalt movement. The third part looks at its further influence on *The Nature of Human Conflicts*. In the following chapters the book itself will be examined as a whole, and then in detail.

I

‘Questions of Principle in Contemporary Psychology’

There is no doubt that both Luria and Vygotsky were highly impressed and influenced by the Gestalt school in the mid-1920s. Even though there were differences of approach from the beginning, Luria and Vygotsky held the leading members in high regard as serious scientists and in the early 1930s they invited Wolfgang Koehler, Kurt Koffka and Kurt Lewin to join Luria's expeditions to Central Asia. Lewin and Koehler were unable to come. After Vygotsky's death Lewin and Koffka, together with many others, not associated with Gestalt theory, such as Adolf Meyer and Karl Lashley, wrote of their admiration for Vygotsky. In 1936 Lewin and Lashley expressed their willingness to contribute a chapter to a posthumous *Festschrift* in his honour (Vygotskaia and Lifanova 1999, IV, 22-3; Zeigarnik, cited in Van Der Veer and Valsiner 1991, 288). Unfortunately the rise of Hitler in Germany in 1933 dispersed the German academic community and closed down areas of research in the same way that Stalin completed the closing down of branches of psychological research in 1936, so that Vygotsky's *Festschrift* never materialized. Another more considerable consequence of the combination of these two events was that it prevented Luria from submitting his and Vygotsky's own developments to such a serious and respected scientific audience, thereby putting back the reception of their full achievements for decades.

In July and August 1925 Luria accompanied his father on a trip to Germany. Luria's daughter reports that they visited Frankfurt, Hamburg and Cologne, but makes no mention of Berlin. She does, however, cite an archivist of German

psychological material, O. Bulgakov, to the effect that Luria did make the acquaintance of Kurt Lewin in 1925 (E. Luria 1994, 43-4). Lewin, though not one of the founders of Gestalt psychology, was already a prominent member of the movement, and from this period onwards he directed a famous group of researchers in Berlin. This must have impressed Luria considerably. Several of Lewin's team were Russian, including Gita (G.V.) Birenbaum, and Bluma (B.V.) Zeigarnik (cf. A. Marrow 1969, especially Appendix B). Both subsequently returned to Russia to work with Vygotsky. Luria met the leaders of the Gestalt movement and the Russians when en route to America in 1929. Zeigarnik, who became a lifelong friend of Luria, reports on meeting this youth, and how Koehler and Lewin were astounded at his erudition (E. Luria 1994, 44). Lewin's theories played a prominent role in *The Nature of Human Conflicts*. Luria introduced Eisenstein to Lewin in 1929 to discuss forms of artistic expression (Homskaya 2001, 39).

In 1926 Luria wrote an article for the Russian journal, *Pod Znamenem Marksizma* (Under the Banner of Marxism), and in 1928 another for its German namesake. Both these articles deal substantially with the work of the Gestalt school. Although Lewin's researches do not figure largely in either, it is clear that the reception of the work already undertaken by such figures as Wolfgang Koehler, Max Wertheimer, Kurt Koffka and Kurt Goldstein marked a sea-change in the ideas of Luria, and also Vygotsky. It removed the apparent impasse that both Luria and Vygotsky had reached, and must have been enormously exciting for them. Therefore I have quoted substantially from these works.

Luria opens his 1926 article, *Questions of Principle in Contemporary Psychology*, by stating that "Contemporary psychology is going through a period of intensified construction and revision of its most fundamental postulates. It can be said that the first stage of this exercise, namely clearing the ground for new directions and systems, has been accomplished" (Luria 1926c, 1).

[At the basis of this lay] the *behaviour of man*, living in the fully determined conditions of society, which became the fundamental subject of the new psychology. The increasing requirements of practical life drove out the tasks of

deep self-contemplation, the study of one's consciousness, and so on. Instead of these what came to the fore was the task of understanding the mechanisms behind human acts, as well as their motives, the forces organizing man's responses to demands set him by the environment, briefly to understand the whole mechanism underlying man's adaptation to the environment (ibid.).

This was apparent in the applied fields of psychotechnics, medical, educational, forensic, social and comparative psychology. This was manifested in two ways: Russian and American psychology focused on how to study human behaviour objectively, whereas the new German psychology, *Gestaltpsychologie*, asked "What is human behaviour and upon which principles should its study be based?" (2). "How has this system of behaviour been constructed? Can this system be divided into separate small elements or does it constitute an integral and indivisible system? Is this system organized according to a mechanical principle or according to one which approximates to the organic?" (3).

"The old academic psychology ... considered that the psyche evolved from separate elementary psychic processes. It knew of various single elements of the psyche such as sensations, feelings, volitional impulses, and so on. From these and by means of associations (combinations) more complex unities of perceptions, representations, concepts, moods, and so on, were formed. Complex psychological processes were thus mechanical groupings of elementary units" (ibid.). Associationist psychology was found severely wanting.

Most recently other systems have surfaced, which whilst operating with materials different from those used by the associationist psychologists, still borrowed from them all their principles and structure. Contemporary reflexology may serve as a typical example. It does not recognize any types of behaviour other than reflexes and conditional formations created on the basis of reflexes (once more resulting from the synchronized convergence of any two agents, even if completely unrelated) and uses them to construct not only the most rudimentary floor but the whole building of psychology. Hence reflexology takes to its logical extreme the propositions of the old associative psychology by asserting that the behaviour of man in its entirety may be reduced to mechanical combinations of mechanically

formed conditional reflexes. That is, what in the old psychology was known as association by contiguity (4).

Thus, within a short space of time, Luria recognized that he no longer had to work within a framework that had seemed to be leading both him and Vygotsky into a theoretical cul-de-sac. Gestalt psychologists, in contrast to reflexologists, set out initially “to study the integral personality, as an organic unity, and not only because these are the requirements of daily life, but because the psyche is an *integral indivisible process*, an integral system of behaviour which is not reducible to its elements” (4-5). As regards any natural process we find most applicable the proposition “profoundly formulated by dialectical philosophy: with the ever growing complexity of this or that process, at each new stage we obtain a new closed system, and a new qualitative unity” (5). Here Luria is clearly referring to the Hegelian approach as made concrete by Marx. Fortuitously, in the same year that Luria made the acquaintance of Lewin – 1925 – Engels’ theoretical work, *Dialectics of Nature*, was published. This is more apparent in Luria’s article from 1928, but the coincidence was fruitful for both Luria and Vygotsky.

What factors of form (*Gestaltfaktoren*) were significant in creating an organic unity as opposed to a mechanical unity?

This relation of man to his environment constitutes the factor defining the moulds into which human behaviour is poured. In the most extreme cases these identified forms of the equilibrium between man and environment are echoed in the sense of changes in the structure of the organism ([Oskar] Hertwig had already demonstrated this with his [response to the] biogenetic law). In less extreme instances no changes in anatomical form are recorded, but we do observe fully defined functions, in other words, a fully defined psyche (6-7).

Here Luria, like Vygotsky, shows his concern with issues of development. Hertwig was famous for arguing against Ernst Haeckel’s theory - that ontogeny recapitulates phylogeny - on the grounds that the similarities were not due to recapitulation, but simply because there was no alternative means for developing a many layered structure from a single cell (cf. Gould 1977, 431, n.29). Here again,

and especially in the arguments below, Luria has set aside the arguments implicit in Freud's *Beyond the Pleasure Principle*.

"In constructing the psychology of personality, we must proceed not from the elementary to the complex, by means of mechanical combinations of elements into unities, but rather the reverse, from the interrelationships between the organism as a unity and the environment down to separate particular processes of behaviour. After lengthy experiments, the American biologist, [Charles M.] Child drew a conclusion, which he most aptly expressed as follows: 'It is not the cells which create the organism, but rather the organism which creates the cells'.

Thus the German psychologists arrived at the conclusion which could be formulated using the paradox that 'the complex precedes the simple', i.e., that the complex system which is an integral organism, taking shape during its interrelationship with the environment may subsequently, and in the presence of appropriate conditions, differentiate itself and develop particular and elementary functions. The second conclusion to which the new German psychologists are drawn, is that there is not and cannot be anything in the organism which does not possess a defined vital significance. That is, which does not result from efforts at maintaining the equilibrium between organism and environment, and which does not answer earlier or current requirements to adapt to vital conditions (Luria 1926c, 7).

Although at first glance reminiscent of teleology, this merely means that "the integral closed system, which includes man, is a system of 'organism-environment' which demands the maintenance of equilibrium between its independent parts... " (ibid.).

Luria then moves on to the relationship between instinct and reflex. Edward Thorndike, for instance, saw instincts simply in terms of 'chains of reflexes', a position also taken by Pavlov's school. For these "the biologically expedient impact of instincts may be decomposed into a series of mechanical reflexes, none of which may possess the character of expediency". Others, including Gestalt psychologists, hold that "instinctive activities possess a complex and integral

character”, namely, that the satisfaction of the instinct may be achieved in different ways as distinct from a simple ‘chain of reflexes’ (9). Luria accepts that it may be valuable to artificially decompose the instinctive act for some purposes, but that it is nevertheless qualitatively distinct.

He cites Wolfgang Koehler’s famous experiments with chimpanzees (Koehler 1921) as an example of an approach which demonstrates that higher organisms need, and are capable of, qualitatively new forms of behaviour, which were not reducible to instincts or simple combinations of habitual reflex responses. “He did not start from a simple stimulation on any part of the nervous system, nor did he place the animals in artificially contrived experimental settings, to artificially isolate reflexes. He decided to exploit *natural-biological, integral settings* in order to elicit ... a complex *spontaneous* act, by placing [the chimpanzees] in challenging circumstances, designed with a specific object in mind” (Luria 1926c, 9). Different results were obtained, and some could not solve the problem – obtaining bananas by using a combination of objects potentially within their visual fields. Agitation was followed by cooling down, then immobility until the successful chimpanzee, [Sultan], suddenly [and famously] and without any further trials headed straight for a number of boxes, stacked them on top of each other and grabbed the fruit. Once achieved, this procedure was never forgotten. “Complex organized behaviour or, as the German writers prefer, ‘intellectual’ behaviour, cannot possibly be constructed from mechanical combinations of simple lower forms of behaviour” (10).

Luria, and Vygotsky, who cited and discussed Koehler’s experiment on several occasions, notably in part one of a joint work with Luria, *Studies in the History of Behaviour* (Vygotsky and Luria 1930a), and in his 1930 introduction to the Russian translation of Koehler’s work, were enormously impressed by it.

[A ‘natural’ experiment based in the real world had the advantage that it propelled] the research of ‘*intelligent acts*’ into the foreground. Not in the sense of whether they are accompanied by consciousness or not, (this question does not in the slightest, interest the German school [a point to note – MH]), but rather in the sense that these acts must possess in the subject’s eyes, a fully defined significance, and

be directed towards the implementation of a given biological or social task for it is precisely of such tasks that the real human behaviour consists (Luria 1926c, 11-12).

The structure of such acts was clearly of a different order from those accommodated within the frameworks of Freud and Pavlov, and Luria's involvement in investigating such structures obviously received encouragement from the research methods of the Gestalt school.

But he was not uncritical. In his conclusions he said, "we must at once recognize that ... [the] method is not devoid of substantial shortcomings". In reality the various experiments he mentions "primarily allow a 'description' of the acts under observation and only the registering of the most general interrelationships. So far the integral approach does not allow any quantitative expression of the phenomena observed and is far from any accurate resolution of the problem". He admitted that these were early days.

However we maintain that these defects are *so far* characteristic of the integral approach to the human psyche... [But] one may think that the dialectical approach to these phenomena will come to the rescue. An approach which takes into account that each new stage of development gives new and utterly original forms of behaviour and reactions of the organism, but which at the same time understands that these new and original forms, skilfully analysed, may be explained by relying on the quantitative changes at their basis (14).

It was to be the contribution of Luria and Vygotsky to apply a developmental framework to the integral systems that Gestalt psychology proposed. Without such a framework and the work that could demonstrate changes in such systems, evident in child development and aphasia, the concept of such systems could be interpreted as perhaps another abstract idealist formulation. The application of approaches traditionally associated with Marxist analysis was to prove truly beneficial for this in the long term.

II

‘Modern Psychology and Dialectical Materialism’

This application of Marxism is made more explicit in the title of Luria’s 1928 article, *Modern Psychology and Dialectical Materialism*.

Psychological science came unconsciously upon the track of dialectics and materialism and needed to grasp this development consciously in order to master them. This development in psychology was expressed in two notable tendencies: in one, ... [American Behaviourism], to change psychology from a science of the subjective mind [*Geist*] into an objective science of behaviour. In the other, ... [Gestalt theory], to change the system that the ‘world of the soul’ mechanistically puts together out of simple associations into a science that investigates unified psychic processes and their laws (Luria 1928b, 508).

Half the article is devoted to American Behaviourism and demonstrates familiarity with the work of J. B. Watson, A. Weiss and others. As one would expect, Luria praises its attempts at introducing forms of objective measurement and recognizes that it has enriched psychology, but its limitations are manifested all too clearly. Its monism was so one-sided, that consciousness had no place in it. Luria (513) cites Edward Tolman to the effect that Watson was virtually reducing behaviour to muscle contraction. Consciousness for Watson can only exist if muscular activity can be found. Thus before a child acquires speech, it cannot be conscious. Luria recognizes that many of the examples he cites would not be considered worthy of serious discussion. On the other hand such approaches are “widespread not accidental and characteristic of the primitive-objective mechanical method”. Furthermore, it “has not grasped the failings of the old associationist psychology”.

[Thus it sought to reduce human behaviour to] a mechanical combination of very simple individual reflexes and instincts. Undoubtedly a standpoint, reduced in such a way, one that asserts that all complicated forms of behaviour arise as a mechanical superstructure [*Ueberbau*] – a ‘conditional’ doubling of primitive reactions – is false and unsatisfactory. The mechanical *Weltanschauung* considers neither the uniqueness of such new forms of behaviour nor the circumstances

whenever they arise, not individually, but as dependent on the biological setting of the whole organism, by which they are organized and regulated (517).

In varying degrees these accusations could also be levelled at Pavlov and Freud.

Gestalt psychology, says Luria, offers “a serious critique of the mechanical *Weltanschauung* and placed itself on a clear dialectical standpoint” (ibid.). Its founders “united around a basic thesis from which they all proceeded: that the complicated processes of psychic-nervous life presented qualitative structures, which could certainly not be reduced in a simple mechanical fashion into constituent ‘elements’... [Only rarely may this be possible], without the whole object losing its identity”. “In most cases the objects of the physical world have their completed form, and this has its own specific laws” (518). “Life processes have their own precise forms which are characteristically unique precisely because of their complexity. These phenomena add in principle nothing new to a range of physical, chemical processes, but are qualitatively distinguished from these in that their Gestalt conforms to the complicated conditions under which they developed” (519).

In all these cases the whole is, of course, composed out of functional parts, but the parts do not determine the whole. Rather, the whole broadly determines the parts... The psychic processes – more so than other kinds of process – are processes of a totality, structurally formed processes. From observant investigations ... we can obtain a conception both of its own structure and of the inner orderliness of these structures. Perception, capacity for thought and behaviour – all give us examples of such structurally formed processes, and to want to comprehend them, without proceeding from the structure, is naturally hopeless (519).

Later (522), Luria praises the work of Kurt Goldstein (1878-1965) for his concrete application of this approach. In a posthumous appreciation (1966, 311) Luria wrote:

There is every reason to regard Kurt Goldstein as one of the founders of contemporary neuropsychology, and every scholar who took part in the development of this new science has felt his influence. I remember the days in the 1920s when I started work in this field, and the deep impressions which I gained

from his basic works, although I cannot agree with most of his philosophical and theoretical ideas... [Specifically], in 1925 a short paper by Goldstein appeared, *Das Symptom, seine Entstehung und Bedeutung* [The Symptom, its Origin and Significance, (Goldstein 1925b)], and this was the start of neuropsychology. The symptom cannot be regarded as an immediate expression of the damaged function: it has to be analysed, and only an analysis of the basic function, which has to be singled out, can show its real essence. This basic disturbance can solve the riddle of the entire syndrome – and only when it becomes clear is the clinical analysis of the patient over” (ibid. 312, cf. also Goldstein 1925a, 398).

The very term ‘symptom’ itself as applied to neurology would today seem meaningless without a conception of structure and development which Gestalt theorists had started to elaborate, though I do not think Luria intended to attribute this change exclusively to Goldstein.

Luria’s 1928 paper turns to look at the nervous system as a whole. Gestalt theorists proposed that

If our perceptions are formed according to a particular structure, that is merely a sign that just such physiological structures are concealed behind. The subjective ‘phenomenological’ forms represent their reflection. These writers accept that if the stimuli are received by the eyes not as elements but as forms, the processes of the retina and in a large part of the cerebral cortex do not proceed in a mosaic-like fashion, but possess an overall, structurally formed character. In the dynamic of the physiological processes we can follow the same regularities as in the field of perception. As with the one, so with the other: it is subjected to apparently unified basic laws in the spread of nervous arousal (Luria 1928b, 521-2).

Precisely such a monist presentation of basic research into perception has brought the scholars of this school to completely new laws about the structure and regulation of the activity of the nervous system. These views basically come down to the perception that the nervous system continuously functions as a whole and that, for the purpose of undertaking any movements and behavioural acts, it does not at all require the premise of specifically located stimuli to be present with a consequent associative ‘connection’. All relationships in which an organism succeeds have the capacity to elicit overall and plastic changes of the organism and its relationships, complete alterations to the whole nervous system. These then (and again in consequence of specific relationships of the situation and the structure of

processes) become localized and altered, are capable of embracing distinct processes, and so forth. In this dynamic of the structure of nervous excitement and, in the opinion of these writers, the same basic laws apply as do in the perception of appearance. Thus for example, in K. Goldstein's view, in the activity of the nervous system, as also in perception, this fact plays a role. The arousal does not extend totally to all sections of the nervous system, [but] according to the 'neighbourhood' and 'appropriateness' to the given excitation. This obtains due to the dominating system and such systems built, which can be designated as the 'ground' (background) for the main process proceeding in front of it. In the structure of nervous excitement such factors play a part as the previous states of the functioning [funktionierend] systems and the residual 'background', the law of inertia in processes already started, the regulation of the levels of tension in processes, the influx of opposing impulses, and so forth. All this creates a highly complicated system of activity, that nevertheless possesses strictly internal laws (522; cf. Goldstein 1925a, 394).

These comments are most intimately related to Luria's work at this time. Indeed he uses Goldstein's term '*Adequaetheit*', literally 'adequacy' in *The Nature of Human Conflicts* on several occasions to refer to the problem of the nervous system under-responding or, more often, over-responding to a given situation. (Gantt's maintenance of the term 'adequacy' in his translation is misleading as it actually signifies 'appropriateness' in common parlance). Luria also used parts of Goldstein's terminology, such as '*Nahewirkung*' and '*Kurzschluss*' (cf. e.g. Goldstein 1925a, 378-9, 388). On the other hand Luria did not make use of the terms 'figure' or 'background' in his own works. He continued to refer to Goldstein's work, 'The Symptom' (1925b), and to another article, *On the Theory of the Function of the Nervous System* (Goldstein 1925a). Rather surprisingly, Goldstein merits only one mention in Luria's book. Luria later wrote:

Goldstein made a heroic attempt to overcome the conflict of strict localization and the mentalistic approach of the noetic school, [which held that only the brain as a whole can be responsible for the complex mental functions]. Although [Goldstein] highly sympathized with the latter, [he] remained to the end of his life a brilliant representative of classical neurology with its analytical approach. The conflict of two traditions was the basic content of his life, the attempt to construct a new

neurology which had to include the truth of both was his endeavour (Luria 1966, 311).

The influence of the ‘noetic’ school on Goldstein was, however, most apparent after he left Germany and, Luria tells us, he lost his intimate connection with his experimental laboratory, where he worked with, among others, Adhemar Gelb and where his cousin, the philosopher Ernst Cassirer, famously observed his work on aphasia. It is this later work which was translated into English, but very little of his path-breaking work. Yet Goldstein never really understood the importance of the investigation of the ontogenetic development of neurological working systems and consciousness. He regarded the ‘higher centre’ as providing a more complicated system which gave humans more scope, but he seemed to see this as a quantitative rather than a qualitative change (Goldstein 1925a, 395). Nevertheless the evidence of Goldstein’s analytical work is apparent throughout Luria’s studies of the structure of nervous excitation and elsewhere. One important point that Goldstein recognized was that although a damaged organism showed “certain similarities with those of a primitive one, it does not thereby ever become a primitive organism”. Or, more forcefully, “A reduced organism is a defective system, a primitive one is always complete” (Goldstein 1925a, 404). In view of the fact that in the late 1920s several psychologists were comparing the verbal and thought processes of children, schizophrenics and so-called ‘primitive’ peoples, this was a significant point.

Luria’s article concludes with another brief look at Koehler’s studies with chimpanzees. Surprisingly he does not examine Lewin’s work. In *The Nature of Human Conflicts* it is Lewin’s work that appears to be the most significant Gestalt influence on Luria’s work. It is to this that we now turn.

III

The Influence of Kurt Lewin

Like Goldstein, Kurt Lewin (1890-1947) also moved away from his early work related to ‘tension-systems’ to the application of field theory to social psychology when he left Germany. Few of his earlier works are available in English translations. But in the 1920s his work was also considered to be particularly

original. In his book Luria refers to three works of Lewin (Lewin 1926, 1927, and 1929). The one referred to most by Luria is *Vorsatz, Wille, und Beduerfnis* ('Intention, Will, and Need'), published in 1926 and available in English in a translation which, however, does omit passages. In section two, 'The Theory of Intentional Action', Lewin raised a number of issues that Luria found relevant for his own work, and significant for its development. The section deals with the consequences of *Quasi-Beduerfnisse* or 'quasi-needs', needs other than those directly associated with drives. These needs were conscious, intentional, often voluntary, though often constrained by cultural influences. They could be as mundane as posting a letter. But they introduced a connection between higher mental functions and the concept of needs, thus moving the discussion about needs on from Freud's concern with instincts to one apparently compatible with conceptions of 'new, historically formed needs' associated with Lujo Brentano, Marx and many others.

When Luria came to write about Lewin's work in his book, he was particularly concerned to draw attention to Lewin's experimental talents. He wrote that "Lewin, in a series of carefully executed experiments, attempted to show a more sharply marked relation between the processes of tension, discharge, and affect" (Luria 1932a, 13). Again, Luria (ibid. 207-8) wrote:

Kurt Lewin, in our opinion, has been one of the most prominent psychologists to elucidate [the] question of the artificial production of affect and the experimental disorganization of behaviour. His method of proceeding – the introduction of an emotional setting into the experiment – helped him to obtain an artificial disruption of the affect of considerable strength. And in his experiments it is only rarely that the affect elaborated experimentally passes over into actual living experience, and the subject begins to feel success in the experiment, in a very broad sense, just as he would in life. Here the fundamental conception of Lewin is very close to ours.

Every elaborated excitation manifests a tendency to a direct discharge (*unmittelbare Entladung*). Obviously, it is precisely the inhibition of this tendency (linked to a specific conflict) that can produce an acute disruption of affect and a series of hitherto unobserved phenomena. The closer the action is to realization, the greater the affective disruption that can be provoked by its inhibition. In this situation, conditionally characterized as the *beinahe Entladung* [imminent

discharge], the inhibition naturally elicits the maximal disruption of affect... The experiments in inhibition of the imperative process lead to confused activity, motor agitation, and finally the complete disorganization of behaviour, whether they are elicited by external artificial inhibition (interrupted activity – [as in the experiments of] Ovsiankina, Isko), or the impossibility of finding a solution to the problem (Dembo). In all these cases, the conflict that appears in a definite phase of activity leads to special forms of disturbance of ordinarily organized behaviour.

Here we see Luria in his element, and the discovery of what one might call ‘fellow travellers’ along his research path must have had an extraordinarily liberating effect on him. (Maria Ovsiankina and Tamara Dembo had incidentally also been born in Russia). In the above passage we find he is so at home in the German terminology that at times he apparently even fails to translate the terms into Russian. But we must return to Lewin’s text. The major part of the section titled “The Theory of Intentional Action” is subtitled “The Effect of the Act of Intending Is a Quasi-Need”. Almost its first words are, “There exists ... an internal pressure of a definite direction, an internal state of tension which presses for the implementation of the intention even if no predetermined occasion invites the action” (Lewin 1926/1951, 114). “The recognition that the driving-force of intentional activity is not an associative coupling [as in associationist psychology], but an internal tension-state – that is, directed internal pressure – makes it possible to explain the phenomena described” (116). There are of course parallels with basic needs – instincts. But there are other reasons for such phenomena. “The objects and events of the environment are not neutral towards us in our role as acting beings. Not only does their very nature facilitate or obstruct our actions to varying degrees, but we also encounter many objects and events which face us with a will of their own: they challenge us to certain activities” (117). This is particularly evident in children. “The development of the achievement abilities of an individual does not depend only on the potentialities of ‘endowment’. For instance, the development of speech or intellectual achievements is basically influenced by the degree and direction of such ‘inclinations’ [both challenges and preferences within them], which are the motors of psychic processes” (120). This whole approach had an enormous effect on Vygotsky as well as Luria. Not only was it a psychology that treated humans as active and acting beings, but it

recognized that challenges could be motors of psychic processes and development.

Vygotsky, like Luria, appears to have been a little slow to make use of Lewin – at least in print. But he certainly found these positions interesting. In 1931 he wrote:

The teachings of interests during the transitional [i.e., adolescent] age may serve as the best illustration of Lewin's position ... that interests cannot be understood outside the process of development, that concepts of growth, crisis, and maturation are the basic concepts in the approach to this problem. It is enough to consider the history of the development of interests at this age to be definitively convinced of how erroneous it is to identify interests with habits and driving forces with mechanisms of behaviour" (Vygotsky 1998, 12).

In the previous pages he also discussed integral, structural dynamic tendencies as well as quasi-needs. But he also recognized that Lewin focused on the developmental aspects of interests and challenges in terms of the structure of the whole field of which they were a part. That is, he grasped everything as a whole within a limited section of time, without including the transformative effects of such situations in his structural whole, where "the structure of the field changes radically". Lewin had proceeded one-sidedly. "Structural [i.e., Gestalt] theory proceeds along the path marked out by Hegel", but fails to grasp the dialectics of change (ibid. 10).

It establishes a series of functional and structural differences between temporary and real needs, but does not take into account the principal difference... In essence, only man in the process of historical development has risen to creating new driving forces of behaviour. Only in the process of the historical, social life of man did his new needs arise, form and develop: the most natural needs underwent a profound change in the process of man's historical development (ibid. 11).

In what one might term Vygotsky's 'retrospective' assessment of Lewin and Gestalt theory as a whole, written from the perspective of 'cultural-historical theory', one needs to remind oneself of Vygotsky's own recent relationship to psychoanalytic thinking. The transition that Vygotsky and Luria underwent was enormous. Undoubtedly their application of Marxism to psychology deepened over the years, but Gestalt psychology, in its widening of their horizons, itself

played a crucial role in breaking them from their earlier positions. I think they were justifiably critical of its limitations from the beginning, but it gave them the confidence and the fresh perspective necessary to embark on their own transformation. The concept of quasi-needs is a case in point.

In his comparison of genuine-needs and quasi-needs, Lewin writes:

Needs imply states of tension which press toward satisfaction. Satisfaction eliminates the tension-state and may, therefore, be described as psychological 'satiation'. The valences that a region of structures and events has before satisfaction ... are eliminated by satiation. The region becomes neutral. Needs and intentions are analogous in this respect... This basic phenomenon of the intention-effect, which a theory of associative couplings can hardly explain without complex auxiliary hypotheses, becomes understandable if the intention-effect is considered to be the arising of a quasi-need and the consummation of the intention to be its satisfaction, that is, satiation (Lewin 1926/1951, 124-5).

With this formulation we see that the neurological disturbances that Luria had been investigating – in particular in relation to intentional and non-instinctive behaviour – did not need to be reduced to some hypothetical drive, because both intentional and instinctive needs produced analogous effects. What became more significant was the structure of the dynamic processes and the causes of its disruption. It was clear that one no longer needed to employ reductionist theories. Practical experimentation, observation and analysis now had to be considered the prerequisites of any theory. Indeed, in the mid 1920s experimental findings were vital to the theories that shattered the old simplistic and reductionist frameworks. Luria could build on these experiments even though he had not yet developed his own theoretical framework. This is evident when Lewin asks, “Is there some genuine- or quasi-need for [a given] case even if the major [genuine-] need is absent? Is it possible that here we have tensions which are intermediary forms between genuine- and quasi-needs and are related to those general goals of will which shape our everyday life: arising, dressing, taking meals, going to sleep? ... Only experimental analysis can answer the question...” (131-2).

There is yet a further major issue that Lewin raised, which Vygotsky in later years regularly referred to.

The relation and clashing of quasi-needs and genuine-needs lead us to the problem of '*freedom*' of intentions. The extraordinary liberty, which man has, to intend any - even nonsensical - action - that is, his freedom to create in himself quasi-needs - is amazing. This is characteristic of civilized man. Children, and probably also preliterate, have it to an incomparably lesser degree. It is likely that this freedom distinguishes man from kindred animals more than does his higher intelligence (136).

And Lewin adds, "This distinction is obviously related to the problem of 'control'" (ibid.), an important consideration, particularly for the future theorizing of both Luria and Vygotsky.

Lewin discussed at some length the relationship between genuine- and quasi-needs. This, he felt, explained the paradoxical result that

The intensity of the act of intending does not decide the effectiveness of the intention... The tensions and valences to which the act of intending gives rise are not primary. They derive from some genuine-needs, which in turn arise from drives or general goals of will. After a *quasi-need* arises from a genuine-need, it still remains *in communication with the complex of tensions implicit in the genuine-need*. [It is "embedded" in the genuine need] (137).

Again this has an intimate connection with Luria's experiments, in terms of the relation of affect to controlled behaviour and its measurement, and the theoretical models applied. Goldstein also talked in terms of systems, of one being 'embedded' within another. This was a far cry from simplistic reflex theory and one that, as we shall see, Luria was quick to develop. The editor of the book in which this translation of Lewin is contained, the psychoanalyst David Rapaport, notes the "dependence on and derivation from hierarchically more fundamental needs" of these intentional needs, but notes that Lewin will below also "emphasize their autonomous and segregated characteristics" (ibid. n.115). Here again it appears that Lewin, though far from rejecting all psychoanalytic work, may have encouraged Luria to move yet further from its intrinsically reductionist positions. Lewin notes, "Whether or not, besides these genuine-needs, there is an individually variable *reservoir of active energy*, used by intentional action not

based on genuine-needs is an open question awaiting experimental exploration” (139). Rapaport (*ibid.* n. 118) may link this question to the Freudian concept of the libido, but to me this leaves all options open pending scientific exploration.

Lewin wrote, “This relation of needs is not merely theoretical, like that which obtains between different yet conceptually comparable types of needs. It is a *real communication* between concrete tension-systems” (138-9). But perhaps more significant in confirming the importance of Luria’s line of research is the moment when a mere wish “crystallizes into a definite quasi-need. The crucial difference [between these two states] seems to be that crystallization of a quasi-need creates, in principle, an avenue to the motor region which did not exist before” (141). Furthermore “the extent to which a psychic event or force influences other psychic structures depends on whether they are embedded in the same or different complexes” (142).

On the other hand, a consequence of learning a new task is often that the task becomes almost automatic. Independent and segregated from the rest of the motor region, it is “tuned together with a previously subordinate part of the perception-basis into an independent action-organism. The kind and intensity of Gestalt ties, of system-relations, undergoes a dynamic change: old bonds are dissolved and a new, relatively closed structure is formed” (143). Where a task is particularly painful to the subject, “the tensions of the quasi-need arising ... are far more strongly segregated from the rest of the ego ... [thus creating] a boundary between this quasi-need and the other psychic complexes...” (*ibid.*). The new couplings are of systems, not associations or reflexes. They allow for dynamic and plastic changes of a different order to previous theories. They can be ‘segregated’, sometimes with special boundaries. This plasticity thus allows for increased differentiation. Another advantage of the ‘segregation’ is noted by Lewin.

It is clear that the essential achievement of the intention is one of preparation. Due to the act of intending, at some subsequent time a psychological field appears which otherwise would not have existed, or at least not in the same form. *Forming an intention creates conditions which allow us later simply to abandon ourselves to the effects of the field* (letter and mailbox), or permit a (psychological) field to be

so transformed, or so supplied with additional forces, that a controlled action becomes feasible or easier (148).

Lewin was truly justified in describing his work as a dynamic theory of personality". The potential of this paper was enormous, and recognized as such by Luria and Vygotsky. It is a tragedy that Lewin limited his future work to the investigation of psychological fields, within a philosophical framework so manifestly Platonic. One speculates that if Vygotsky had lived, and reactionary regimes had not driven them all apart or even suppressed their work, the history of psychology could have been very different. As it transpired, it was not Lewin, but Luria and Vygotsky who developed the theoretical potential here.

Towards the end of *The Nature of Human Conflicts* Luria assesses the problem of the concept of quasi-needs and its importance in the development of his own work. Lewin and others consider voluntary acts to be automatic, like those of a machinist.

They claim that these automatisms direct the free 'I', and they posit that the executive of these automatisms is necessity, inclination, emotion... The primitive hedonistic theory of will does not convince us. We cannot believe that the will is such a 'slave of passion' as is represented by this system. The behaviour of the social human is sharply distinguished from the animal in this: it is often directed towards overcoming necessity, inhibiting it, controlling it. The complex forms of human labour presupposing 'will expressed in attention' (Marx) [cf. Marx 1976, 283-4] cannot be understood as a simple discharge of the tension created by necessity. The researches concerning the mechanisms of 'artificial' necessity, i.e., *Quasi-Beduerfnisse*, help us to understand the mechanics of the voluntary processes better, but they fall far short of telling us by what means the human establishes new requirements. And they do not explain the process of human voluntary action in all its specificity" (Luria 1932a, 399-400).

Nevertheless, the study of *Quasi-Beduerfnisse* put us on the right path in resolving these complicated questions. In view of the fact that the 'voluntary mechanism' is a mechanism of subordination, especially via artificially created stimuli that may replace natural necessity, this theory makes a real step forward in the scientific understanding of this problem.

Even though the existing mechanisms enter into the composition of the ‘voluntary act’, this theory does not however disclose the specificity of the resulting behaviour. The mechanism of the *Quasi-Beduerfnisse* and subordination to them are component parts of the voluntary act, but they are always something extra, because they are not *specific* [my emphasis-MH] for the resulting activity. When the child is interrupted and prevented from completing what he is doing, he returns to it thanks to that tension that is created in the broken structure of the action, then this is completed by virtue of the *Quasi-Beduerfnisse*... (400).

(Here Luria refers to Ovsiankina’s experiment. An analogous experiment by Zeigarnik showed that memory was greater in respect of interrupted and hence uncompleted actions – the so-called ‘Zeigarnik effect’). In this whole passage, it is impossible to fault Luria’s logic: the process involved in the quasi-need facilitates the completion of the activity, but not the concrete and specific processes involved in its formation and operation. He is not prepared to explain higher mental processes as epiphenomena of lower processes. He is, however, willing to accept a joint involvement in terms of systems.

This then leaves the question of the role of artificial stimuli in the origin of specific willed acts, and allows us “to follow the investigation along the path of genetic development. The question that arises is this: what is the origin of such artificial needs and of the subsidiary internal stimuli which distinguish the human from the animal, and, to some extent, the child from the adult?” (ibid.). There is a great difference between ‘natural’ needs “and those complex forms of behaviour characterized by the ability to create and make use of the *Quasi-Beduerfnisse*. The difference is what primarily distinguishes the human from the animal, and the fact that the humans are able to control not only the external world but also their own behaviour. This is achieved indirectly by the creation of artificial needs, and stimuli produced artificially especially for the purpose. This is a cardinal factor in the development of behaviour.

“We have good reason to believe that such behaviour is a compound product of psychological growth, in the process of which the primitive, natural forms of behaviour are made more complex by new cultural ones. As a result of this a new relation of the personality to its own behaviour is created” (ibid. 401). At this

point Lewin's concept of the individual's control of his own behaviour is joined to that of Herder, Marx and others who say that this control may be conscious, because it is mediated and facilitated by language and other tools.

The late Eckart Scheerer (1980, 124) considered Luria's two theoretical articles in *Under the Banner of Marxism* to be the high point of Gestalt theory in Russia. It is also notable that Luria's three major articles published abroad on the combined motor and verbal response method were not published in any psychoanalytic journal, but in German academic journals, one of which, *Psychologische Forschung*, was the journal of the Gestalt movement (Luria 1929a). Certainly he used terminology used by Goldstein and Lewin such as 'coupling', '*Nahewirkung*' and '*Kurzschluss*' in *The Nature of Human Conflicts* (cf. II, ii). There is no disputing the enormous impact that Gestalt theory had in shifting the perspectives of both Luria and Vygotsky. Scheerer (1980, 127) adds:

Perhaps the most extensive use of Gestalt ideas is found in Luria's work employing the 'combined motor method', involving the combination of verbal and motor responses to one and the same stimulus. The method is justified by the assumption that it results in the merging of the peripheral (motor) and central (verbal) components in one unitary functional system.

This last point is an oversimplification. The motor components are central to the dominant hand, peripheral for the non-dominant. The relation of the centre and periphery is, however, crucial to *Anokhin's* theory of functional systems. Since I have already noted in the preface that Luria frequently propounded Anokhin's version as a cover for his own, this may account for the misunderstanding here.

[Scheerer Continues:] It should be noted that both the term *Funktionssystem* and the concept, as used in Luria's early work, are borrowed from Kurt Goldstein, the outstanding representative of Gestalt theory in neurology. The results are interpreted in a language borrowed from Lewin ('functional barrier', etc.), but transposed from the phenomenal sphere of the 'living space' to the physiological sphere – once again an instance of the 'organismic' bias in the [Russian] reception of Gestalt ideas.

Van der Veer and Valsiner (1991, 175) also note, "The functional systems in the brain, which Kurt Goldstein's holistic perspective had charted out in his

neurology, provided Vygotsky with much material for thought”. Unfortunately none of these writers provide references (and, of course, Van der Veer and Valsiner typically omit all mention of Luria’s contribution to Vygotsky’s work).

My own attempts to confirm the above statements by a trawl of Goldstein and Lewin’s contemporary writings have proved interesting, and do not exactly confirm them, but that may be due to the limits of my search. Certainly there is enough material quoted above to suggest a strong possibility of their validity – e.g. with the use of ‘functioning systems’ and ‘boundaries’ and also ‘functional relation’ [*Beziehung*] (Goldstein 1925a, 398). Certainly Lewin’s remarks about couplings of systems to form a new system greater than its parts is of fundamental importance. Although, strictly speaking, the term ‘functional barrier’ as used in *The Nature of Human Conflicts* refers primarily neither to ‘living space’, nor neurology, but to the role of cultural influences, its role is intimately connected to neurology, and it almost certainly derives from Lewin’s work.

But it is different in the case of the concept of functional systems. As Goethe once said that there is no fact without a theory, one might add a corollary that the significance of a concept depends on the overall theory of which it is a part. It is of fundamental importance that functional systems in Luria are not used within a Gestalt framework of figure and background, such as used by Goldstein to the end of his life (cf. 1925b, 144: 1967, 156). This very fact suggests a very different perspective. Although Scheerer correctly uses the term *Funktionssystem* with reference to Luria (cf. Lebedinsky and Luria 1929, 474), I have not found it in the Gestalt theorists. It implies a system of different functions, that may not be conceived simply as one system embedded in another, but something to be analysed in its own right – as interactive both internally and externally. At the end of Chapter VII we will look at the development of the term, but from the outset, even if only implicitly, there is a difference in emphasis in Luria’s use of this term.

A colleague from Luria’s later years, Elkhonon Goldberg (1990b, 5), explained Luria’s achievement.

Luria was able to go beyond the simplistic brain-behaviour dilemma of the first half of the century: narrow localization (phrenology style) versus equipotentialism. Luria formulated his concept of functional systems, which were neither. He was probably the first to state explicitly and succinctly that (a) behaviours, skills and traits as they appear in the lay, real-life nomenclature are not the units appropriate for cerebral localization; (b) the identification of localizable elements of cognition is in itself a trivial task; and (c) the relationship between the 'lay' and the 'localizable' nomenclatures is not a simple one-to-one mapping. [In the light of the previous discussions, the attribution of being first is something we would dispute - MH]. In Luria's terms, a behaviour or trait is the product of the interaction of many cognitive elements, each mediated by a different brain structure or region. Such a constellation of interacting brain structures, each mediating a particular cognitive dimension, constitutes a distinct functional system. Conversely, any given cognitive dimension enters a variety of behaviours and competencies as defined in terms of the lay nomenclature. Furthermore, according to Luria, externally similar behaviours can be controlled by differently composed functional systems with different cerebral representations in different individuals, or even in the same individual at different developmental stages.

Naturally these last points were to prove central to many attempts to rehabilitate those suffering from brain damage. While Scheerer is correct in implying that Luria did not reach these conclusions all in one go, it is also true that such a theoretical outcome was a possibility fairly early on in Luria's development. It is also true that such an outcome replaces Goldstein's vacillation between classic neurology - even when extended to the analysis of systems - and Gestalt's overall approach. Luria also goes beyond Lewin in recognizing the role of consciousness in activity. His functional systems extend to the higher mental functions as well as the lower. His version of functional systems almost constitutes a theory in itself, though since it involves fairly open on-going exploration rather than a simplistic reductionism, it is not a 'grand' theory, but a scientific 'approach' to studying the structural dynamics of the brain and behaviour. Luria certainly never presented it as a 'grand' theory, and later even attributed it to Anokhin. Yet it was also a theoretical prerequisite for the scientific investigation of Vygotsky's view of human psychology.

IV

Moving Beyond Pavlov and Freud

It remains to remove, as promised, any outstanding doubts that Luria had moved on once and for all from the simplistic approaches of Pavlov and Freud, though accepting the valuable stepping stones their work had either provided or generated. Given the transformation in Luria's thinking evident in this chapter, the following comments relating to Pavlov should be taken merely as a postscript.

At the 1929 Ninth International Congress of Psychology both Luria and Pavlov attended. Karl Lashley (1890-1958), the American neurologist, gave the presidential address. Although an associate of Watson and promoter of the objective method in behaviourism, he too had moved on from reflexology. His address, titled *Basic Neural Mechanisms in Behaviour*, contained discussions of great interest. "The units of cerebral functions are not single reactions, or conditioned reflexes as we use the term in America, but are modes of organization. The cortex seems to provide a sort of generalized framework, to which single reactions conform spontaneously, as the words fall into the grammatical form of language" (Lashley 1930, 203). "This unity of action seems to be more deeply rooted than even the structural organization." Indeed such phenomena as compensation "suggest that the nervous system is capable of a self-regulation which gives a coherent logical character to its functioning, no matter how its anatomical constituents may be disturbed" (ibid. 204). There is some truth in this. But Luria was later to criticize Lashley and Goldstein for their overemphasis on overall plasticity and overall organization as an extreme reaction to the limitations of localization. There were, however, many shared perceptions also. Lashley was also interested in central excitation, irradiation, and the effect on the motor cortex, though his own experiments were primarily on non-human animals. Luria was particularly impressed with his critique of localizationist positions and the limitations of reflex theory.

On the second page of his book, Luria (1932a, 4) praises Lashley's "brilliant criticism of the model of the telephone station" in reflexology. Lashley wrote that although reflexology was "a welcome addition" to neurological theory, "the

model for the theory is a telephone system. Just as two instruments can be connected only by certain wires, so the sense organs and muscles concerned in any act are connected by nerve fibres specialized for that act” (Lashley 1930, 192). Even if stated more subtly with “the interplay of inhibition and facilitation” and so forth “the fact remains that the essential feature of the reflex theory is the assumption that individual neurons are specialized for particular functions. The explanatory value of the theory rests upon this point alone...” (ibid. 192-3). Pavlov had used the analogy of the telephone station in 1909 (Pavlov 1928, 123) as a way of proposing the advantages of temporary connections over what we would now call hard wiring. But clearly the demands of neurology had moved on.

Pavlov may well have been stung by these criticisms, and also by Luria’s repetition of them. Although Luria described Pavlov in *The Nature of Human Conflicts* as a “great physiologist” (1932a, 12), Pavlov gave the work a hostile reception. Luria’s attempt to present Pavlov with a copy of the book in recognition of the debt he owed to Pavlov was rebuffed.

Towards the end of his book Luria writes:

The external similarity and astonishing syncretism of thinking has induced many authors to correlate the reactive process of the human with conditional reflexes. Only the presence of stimulus and motor response is common in the two cases. While the conditional reflex is formed by the union of the unconditional stimulus with the conditional signal, the simple psychological reaction is elaborated on the basis of speech and includes the higher symbolic mechanisms in its genesis and social character. Though the conditional reflex requires many reinforcements of the unconditional reflex, the psychological reaction is elaborated at once and does not need any support; it does not obey the law of extinction, and on further repetitions it is only strengthened the more... There is reason to believe that in the two cases we are dealing with different mechanisms (Luria 1932a, 388).

The normally reticent W. Horsley Gantt, translator of both Pavlov and Luria, felt constrained to append the following footnote: “For an exposition of the opposite view, that words themselves are but conditional stimuli, one must consult Pavlov and his pupils” (ibid.).

In the case of psychoanalysis and Luria, it is more a question of resolving misunderstandings.

Firstly, in his groundbreaking book, *Psychology in Utopia*, Alex Kozulin correctly states that Luria's 1932 article, *The Crisis of Bourgeois Psychology* (Luria 1932b), briefly criticized his youthful fascination with psychoanalysis. He then suggests that this was a cunning ploy to obscure the fact that simultaneously, in *The Nature of Human Conflicts*, Luria "preserved a limited psychoanalytical approach" (Kozulin 1984, 89). *The Crisis of Bourgeois Psychology* largely repeats criticisms of certain western psychologists that Luria had already made, including the criticisms of Adler and Freud from 1926 and 1928 that we have already dealt with (cf. IV, i, 96). It is true that the article was couched in terms appropriate to someone trying to placate the authorities. As someone who saw himself as a member of an international scientific community, and personally averse to engaging in polemics, he would no doubt have preferred to avoid this form of article, but the psychological discussion was completely consistent with Luria's previously stated views. Although an attempt at thorough editorial consistency in *The Nature of Human Conflicts* was not possible due to the rushed nature of its publication and its conveyance to the publisher in parts, those rare occasions where positive comments on psychoanalysis do appear are usually of a strictly scientific nature, though occasionally they seem anomalous. An alert editor would quickly have clarified the latter, though followers of psychoanalysis will, I hope, be pleased that in the following two chapters I do discuss references to psychoanalysis in addition to those that may be located via the book's index.

Kozulin also cites Luria's views, quoted by Levitin, to the effect that "Luria was at least partially 'forced' from psychoanalysis. Luria claims that Kornilov refused to publish his article, *The Experience of Objective Psychoanalysis*, which was already in proofs, and after that their relations cooled" (ibid.). What Levitin actually writes is that Luria, when talking about his book, said, "This, by the way, was the occasion of my last dispute with Kornilov. He suddenly made a show of his power and forbade me to publish my article, *An Experiment in Objective Psychoanalysis*, which was already being copyedited. The word *psychoanalysis* sounded terrible to the director of this institute" (Levitin 1998, I, 73; previously

Levitin 1978, 54). Since Kornilov had been stripped of any influence well before the publication of Luria's book, the incident almost certainly refers to the period around 1925 when Luria was working along these lines (cf. II, iii, 66). It would in part explain his early disagreements with Kornilov (cf. II, I, 48-50). Presumably the experimental results were incorporated into the book (minus the psychoanalytical theory), which would explain why Luria recalled it in this context. The chapter on the use of hypnosis to artificially create affective complexes (Chapter 4) is almost certainly a presentation of this missing research. Not only was the majority of the work done in 1924-5, but Luria even describes it, admittedly in quotation marks, as a form of "experimental psychoanalysis" (Luria 1932a, 150, also cf. VI.i).

Secondly, Van der Veer and Valsiner (1991, 188) claim that one reason publication of Vygotsky and Luria's *Studies in the History of Behaviour* was delayed beyond 1927 "was that Vygotsky was extremely dissatisfied with the first variants of his co-author's chapter on child development. They contained far too many – uncritical – references to work done by psychoanalysts such as Vera Schmidt, Melanie Klein and others for his taste (Vygotsky in letters to Luria dated July 26, 1927 and Leont'ev, dated July 23, 1929)". The relevant passage of the letter to Luria is unpublished, but that to Leont'ev is available. Here Vygotsky says that the chapter "is wholly in accord with the Freudians (actually, not in accord with Freud, but with V. F. Schmidt in terms of its content, and with Melanie Klein and other stars of second magnitude). Further, [Jean] Piaget [1896-1980], who absolutized beyond all measure, is the stumbling block". Despite this and other complaints "this is not something for which Luria is personally to blame: it is rather the entire epoch of our thought that is at fault" (cited in Vygodskaja and Lifanova 1999, II, 5).

The early draft of Luria's chapter probably owed something to a talk Luria had given to the Psychoanalytic Society on 23 February 1927, *On Experimental Research on the Primitive Mode of Thought in Children* (Schmidt 1927). The published version of Vygotsky and Luria's joint work contains verbatim reports of children's speech cited from works by Schmidt, Klein and others, but no exclusively psychoanalytic positions. But the *principal source of Vygotsky's*

irritation related to some of *Piaget's* ideas about child development. Piaget was admittedly under some psychoanalytic influence himself – he was analysed by Sabina Spielrein (cf. Etkind 1997, 162ff). He held that children were initially egocentric and their speech was more like an autistic monologue than a social dialogue. Vygotsky held that children were initially social and took time to become individualized. For him so-called ‘egocentric speech’ was evidence of the transitional process leading from the social to the social individual. The child spoke aloud about itself, its activities and plans. Subsequently it largely internalized what Vygotsky recognized as a planning process.

Piaget's position was consistent with Freud's theory of the pleasure principle, but Vygotsky's frustration with Piaget was not so much that he was importing views into child development theory that were specifically psychoanalytic, but that they denied the initially social nature of the infant. Luria actually adopted Vygotsky's position on ‘egocentric speech’ in his chapter (Vygotsky and Luria 1930a, 99). Indeed when Vygotsky was criticizing Luria, the latter was in America. He delivered a paper by Vygotsky and himself on *The Function and Fate of Egocentric Speech*. This concluded that Piaget's understanding of egocentric speech as “an expression of the general autistic attitude of the child” had been found wanting in their own research. Here external speech preceded egocentric speech, which in turn preceded internal speech. Egocentric speech had “a specific organizing function”, where the child tried “to solve the problem verbally, in order to organize its subsequent activity”. It is an important developmental process “having a specific function in the evolution of the cultural behaviour of the child” (Vygotsky & Luria 1930b, 464-5).

We do not know when and how much Luria amended his chapter of their joint work. Luria did, however, discuss other ‘egocentric’ aspects of infant behaviour (Vygotsky & Luria 1930a, 99ff). This may well have incurred Vygotsky's irritation, because it failed to take account of the socially interactive nature of the infant and its carers. Like Vygotsky, Piaget, Edouard Claparede, Wilhelm Stern and many others, Luria was concerned to differentiate the stages of a child's development, and in particular to understand the changes that developed from early – or, as it was then called, ‘primitive’ behaviour. This initially evoked many

primitive *theories* from among psychologists and most of these were arrived at independently of psychoanalysis. We shall deal with this in Part III. But it should be noted that while virtually all psychologists, including Luria, had some naïve ideas about this field, Luria was not necessarily investigating the same areas as Vygotsky. His investigations led him within a short period to draw important conclusions about the direct and impulsive character of the infant's behaviour as expressed in its thinking. Here he was concerned not with Piaget's distinction between egocentricity as opposed to sociality, but with the development of self-regulation of the child's behaviour (Luria 1932a, 358-9). This we will examine shortly.

Thirdly, there is the question of Luria's work with the Psychoanalytic Society. Luria was actively involved in it at the beginning of 1927, though the above-mentioned talk was the only one he gave of work related to his own. Vygotsky also gave a talk on the psychology of art in Freud's work. But on 7 April 1927 Luria resigned as secretary of the Society and was replaced by Vera Schmidt. Subsequent reports of the Society's activities were limited and infrequent, but neither Luria nor Vygotsky figure in them. Nor does their friend N.A. Bernshtein appear, though he had been active in 1926. [NB. The tantalizingly brief report of a talk he gave could be seen as another instance of the significance of functional systems. "A sharp distinction must be drawn between form and scheme. The first is determined quantitatively; the second qualitatively, or topologically" (Luria 1927b)].

It is therefore somewhat surprising that in 1929 a membership list appeared including all three (List of Members 1929). It is clear, however, that this is an earlier list introduced to substitute for the lack of reports. Mikhail Reusner is included though he had died on 8 August 1928. The list does omit Moshe Wulff, the former president, who had emigrated to Berlin in November 1927. Therefore it seems likely at first sight that the list dated from the end of 1927 or beginning of 1928. However, given the appearance of Luria, Vygotsky and Bernshtein on it, dating it to the beginning of 1927 seems more plausible. Wulff's emigration was significant enough to be adjusted for. Reusner received an obituary from the German psychoanalytic journal ('Prof. M. A. Reussner' 1929), but not from the

English journal. Since it was the latter that published the list, it may explain why that journal failed to make an adjustment for Reusner's death. Although evidence may turn up to contradict this in the future, at the moment there is no reliable evidence that Luria took any part in the Psychoanalytic Society after his resignation, or that he even remained a member.

There remain those who argue that Luria's later theories show evidence of psychoanalytic thinking. Similarities are inevitably bound to appear – not least in the minds of psychoanalytic writers. On the other hand I think the developmental approach to Luria's theory that I have undertaken would suggest otherwise. It is certainly true that Luria would never abandon practical or theoretical approaches if they were useful or had partial explanatory value. The suggestion that his treatment of patients as individuals throughout his life was influenced by psychoanalysis is certainly plausible (cf. Sacks 1990). On the other hand the fact that his father stressed the importance of doctor-patient relationships and the dangerous implications of iatrogenic disorders suggests that Luria junior may already have had a role model in this respect.

There are many articles devoted to Freud's high estimation of the work of John Hughlings Jackson (1835-1911) and his attempt to apply it to his own theories (cf. e.g. the references in Sulloway 1979). In the early 1930s Luria and Vygotsky made use of Jackson's ideas. They adopted, though critically, his approach to the development and organization of the brain. Jackson, through his reading of Herbert Spencer, had metaphorically applied to neurology the Hegelian notion that the functions of levels were transcended at each stage of ontogenetic development through their incorporation into higher levels. This led to the implication that, after the disablement of higher cortical functions, the lower ones were unable to reassume the form of their operations at a previous stage of development. Or, as he put it, "Scarcely ever, if ever, do we meet with a case of dissolution which we can suppose to be the exact opposite of evolution" (Jackson 1932, II, 47).

There are those who correctly hold that Luria used Jackson's ideas, but tend to present a theoretical line of legitimacy comprising Jackson, Freud and Luria (cf. Solms and Saling 1986, 412). Since virtually all major neurologists including

Head and Goldstein were similarly influenced, but not of a psychoanalytic persuasion, this form of argument is unsatisfactory. This is especially so, given that Freud's use of Jackson was in combination with Haeckel's biogenetic law that ontogeny recapitulates phylogeny. In *Beyond the Pleasure Principle* he then applied this eclectic mixture in reverse fashion to account for neurotic disorders. Unfortunately Freud ignored Jackson's central assumption that the process of dissolution was different to that of evolution. Although it was impossible for Freud to ignore this in his practical work, his application of Jackson's ideas in combination with those of Haeckel in his 'grand theory' was perverse. As we have seen in section II, Goldstein supported Jackson on this point. We shall see later how Luria used Jackson (cf. VII, ii, iii).

Goldstein in later years summed up the problems of the approaches current at the beginning of the 1920s. "Our observations of normal and pathological behaviour have taught us that the activities of the organism cannot be understood as effects of fixed patterns of reaction to stimuli coming from the outside or inside, as was generally assumed in mechanical concepts, such as reflexes, drives, instincts, or will" (Goldstein 1967, 157). He adds (ibid. 158) that, although Freud was at times close to putting "the person into the centre of his concept of sickness and therapy," his "use of it was more than doubtful" precisely because of his introduction of these mechanistic concepts. In retrospect these passages explain why it seemed so natural for serious scientists to attempt to link reflexology and psychoanalysis. Both reflexes and instincts are unarguably real and material, and this was the assumption of Luria in his 1925 article, *Psychoanalysis as a System of Monistic Psychology*, with its reliance on internal and external stimuli. But as we saw with Adler in 1909 this led to a form of reductionism where the ideals of socialism were reduced to aversion to soiling. Psychologists recognized the need to move on to more sophisticated analyses and theories.

This conclusion seems to be shared by two recent psychoanalytic articles on neurology and the role of language. Wilson and Weinstein (1992) consider that it is crucial for psychoanalysts to study Vygotsky's theories in relation to the mind. Solms and Saling (1986, 413) accept that, while "Freud remained aloof from developments in neuroscience", its development "under the leadership of Luria ...

has the potential to offer new knowledge and insights to psychoanalysis". While these writers' views may be far from mine, and fall somewhat short of recognizing the need for a new overall theory, there is recognition that Vygotsky and Luria developed new and important scientific approaches *independently* of psychoanalysis.

Chapter 6: 'The Nature of Human Conflicts' – the Book

Experiments and Theoretical Advances

I

A Brief Overview and Summary of the Book

In the introduction to Chapter 5 we have already described how Luria's manuscript arrived in America over a period of one to two years and in at least two parts. We have also given the translator's account of how he dealt with the manuscript. *The Nature of Human Conflicts* is the title of Luria's American publishers, Liveright. "My title was *Affect, Conflict and Will*, but the publisher thought it sounded too technical and suggested instead *The Conflict of Human Nature*. That sounded catchy but did not reflect the content of the book. So I suggested the words be shifted to make *The Nature of Human Conflicts*, and that was how it appeared in English" (Luria, cited in Levitin 1982, 159). The title page adds "or Emotion, Conflict and Will", together with a subheading "an objective study of disorganization and control of human nature" (Luria 1932a, iii).

Luria's book contains both theoretical and methodological considerations and reports of a great number and variety of experiments. We have looked at two reports so far. It is important to list and briefly outline the experiments first so that it becomes possible not only to grasp the enormous compass of the work, but to relate the dating of the experiments and their writing up to specific theoretical developments. This seemingly obvious and necessary task does not appear to have been undertaken since its first publication. The theoretical developments become apparent *in the course of Luria's exposition of his work*. I add various comments and points of clarification throughout, and at the end of the following chapter.

The first dated experiment took place in the spring of 1924, but other individual experiments or examinations of individuals may have taken place earlier. Certainly the preparations for the experiments themselves occurred earlier and confirm Luria's statement that the research was carried out largely at the Institute of Experimental Psychology during the period 1923-30 (Luria 1932a, ix). The last dated experiment is given as May 1930 (*ibid.* 390).

Although Luria's pre-Gestalt influences do show through at times, Luria states (ibid. 47) that the discussion of the first experiment was based on an article (Luria & Leont'ev 1926) from the period when he began to be influenced by Gestalt theories. The work does show the influence of the Gestalt school to some degree in many chapters. On the other hand the experimental design for this first experiment clearly predates this, and the book is a working out of Luria's own ideas, which naturally builds on the work and ideas of many others including, in the later experiments, those of John Hughlings Jackson, as presented by Henry Head, Head himself, and Vygotsky.

Luria gave most of the work to his American publishers in the summer of 1929. He completed the remainder in 1930 or 1931. He writes of a forthcoming book by one of his collaborators, M. S. Lebedinskii (Luria 1932a, 333). This book, with Luria's preface (cf. Luria 1931a), appeared in 1931. It is relatively easy to distinguish the later sections *The Nature of Human Conflicts* from those handed over in 1929 by the dates of the references, and from the changing focus of Luria's research. In a paper written in 1928 and published subsequently (Lebedinsky & Luria 1929), the authors used the term 'functional system', but not the term 'functional barrier'. In the comparable part, Chapter VIII, Luria does use the latter term. Therefore I assume that chapters using this term were generally written or amended from 1929 onwards. But this should only be taken as a rough guide, which may be superseded when more Russian material becomes available.

Firstly we present a brief introduction to the parts, the chapters, their titles, pages, and, where such data are known, dates of publication of related papers, dates of experiments, and known collaborators. Most of this detail comes from the text itself. The references to related papers are not comprehensive, as we do not have reliable information about some of the Russian papers. Additional comments are given in some cases. Subsequently we will comment on the experiments not already discussed, briefly in some instances, in more detail in others.

Summary of the Contents of 'The Nature of Human Conflicts'.

The three parts of the book basically follow three phases of Luria's work. The 'Author's Preface' (ix-xi) looks briefly at the organization of the book and is quoted extensively in this section. The *Introduction* contains Chapter I: *Problems of the Disorganization of Behaviour* (3-39). Both these pieces were written after the completion of the remainder and provide both a guide to it and attempt to show how the three parts are linked.

Part I, The Psychophysiology of Affective Traces, examines the disruptive effects of emotion on normal behaviour. The dramatic and almost unprecedented nature of the experiments with people reacting in real-life situations ensured that this became the most frequently cited part of the book. The accounts of the experiments are given at some length, whereas those in the succeeding parts are briefer and serve a more illustrative function. Here the accounts describe Luria's earlier experiments, which were the first to be written up. They serve as an essential prerequisite for the advances shown in the succeeding parts. In his preface Luria writes:

The chief problems ... were [to provide] an objective and materialistic description of the mechanisms lying at the basis of the disorganization of human behaviour and an experimental approach to the laws of its regulation. The first of these tasks forced the author to investigate the whole series of phenomena in which the disorganization of human behaviour was clearly expressed: the problem of the diffuse, acute affect, of trauma and neurosis. An analysis of these states and the description of the symptoms characterizing acute affect, as well as its traces, are given in the first part of the book (Luria 1932a, ix).

Central to its approach is the recognition and concentration on the fact that there is such a thing as a structure to the process of the control and organization of human behaviour, which was to require further examination in Part II.

In Chapter II, *The Investigation of Mass Affect* (43-76), Luria states that this chapter is based on the published articles mentioned below. Section A,

‘Experiments in a Situation of “Purgation”’, reports on experiments undertaken at Moscow University in the spring 1924 with A. N. Leont’ev (cf. Luria and Leont’ev 1926). It sought to chart the course of the dynamics of the collapse of higher regulation in cases of ‘mass affect’. It could compare these with normal responses and less severe responses that could be closely correlated with the stimuli presented. It thereby also confirmed the value of Luria’s experimental method. Section B, ‘Experiments in a Situation of School Examination’ took place in the autumn 1927, also with A. N. Leont’ev (cf. Luria and Leont’ev 1929). This in addition compared the response of groups of ‘normal’ and ‘labile’ subjects, finding genuine differences. Luria also thought that there was a distinct similarity in structure of these temporary disturbances to neurosis.

Chapter III, *The Investigation of Affect in Criminals* (77-127), involves experiments that took place over five years, though the cases presented cover the period 1925-7. Lev Sheinin (a detective), doctors and assistants collaborated (cf. Luria 1927a, 1928a, 1930a). It also aimed at uncovering the structure of the process involved in both ‘mass affect’ and less severe disturbances. It had predictive value in the case of the latter: these responses could be correlated with specific verbal stimuli related to the cases in which they were suspects. Luria’s work was not part of the legal evidence, but the verdicts of the court tended to support his conclusions from the evidence of individual cases.

In Chapter IV, *The Investigation of Complexes Produced during Hypnosis by Suggestion* (128-68), the majority of the experiments discussed took place in 1924-5. Luria says some continued after the death of B. E. Varshava, his collaborator, in 1927, though he may be referring to the experiments in Chapter VII. Other collaborators in the hypnosis were Dr. G. Z. Iolles (?) of Paris, Dr. R. V. Valenich of Moscow, and V. I. Zabrezhnev of Leningrad. These experiments involved the use of hypnosis to artificially insert an affective complex into a subject connected with a supposed action of that subject. Luria described the dynamic changes in behaviour, as the subject, under the stimulus of selected verbal stimuli, gradually became conscious of the complex. Initially unaware of the nature of the complex, the subject tried to reject the implications of the stimuli, thereby displacing the complex from the consciousness to avoid connecting with

the motor system. This proved impossible as awareness dawned and motor behaviour disintegrated. Full awareness and comprehension ultimately led to a resumption of normal behaviour.

Chapter V, *Some General Features and Mechanics of the Affective Processes* (169-201), discusses some of the implications of the work. Luria concludes that affect occurs where separate systems join. “The degree of expressiveness of this or that system *depends not so much on its anatomical position as upon its inclusion in one or another complicated psychological structure*” (172), that is, when its functional role begins to be connected with activity.

Part II, The Psychophysiology of Conflicting Processes, reports on experiments with both artificial conflicts and artificial neuroses. It attempted to analyse both the structure and dynamics of the conflicting processes that were thereby induced. These were influenced initially by the experiments of Pavlov and his school with animals and subsequently by the experiments of Lewin and his Berlin team with humans. Here the aims were more precise. The disruptions induced were specifically targeted to examine in more detail the processes and mechanisms involved in the control and disruption of behaviour and therefore did not involve either the dramatic real-life situations or the trauma that accompanied ‘mass affect’. This change was a direct consequence of Luria’s earlier observations.

The study of affects and neuroses and their psychophysiological mechanisms suggested ... that it was not possible to seek the causes of the affective processes in the peripheral apparatus, but that the affective disorganization of behaviour was connected primarily with central changes – with the disturbances of human activity and consequent profound changes in all the systems of psychological function, the correlations of which are fundamentally modified during the state of affect. To accomplish [similar effects experimentally] it was necessary to artificially create affects and models of experimental neuroses which made possible an analysis of the laws lying at the basis of the disintegration of behaviour. The experiments with artificial conflicts, outlined in the second part of the book, constitute an approach to the psychological structure and dynamics of affect (Luria 1932a, ix).

Chapter VI, *Experiments with Artificial Conflicts* (205-38), used two forms of experimental design – the conflict of setting and the conflict of defection – to elicit different forms of delays in activity. The former presented subjects with a difficult choice between two close options. The latter led a subject to believe that a difficult task was possible, when in fact there were not only deliberate diversions, but the task ultimately proved to be unachievable. In the former the delay occurred after the formulation of the intention to act. In the latter the delay occurred earlier, thereby making it possible to transfer conflicts from the motor sphere to the ‘connecting sphere’, i.e., where options are considered and prepared, and thereby isolate them from the motor sphere. Other verbal tests and comparisons of healthy and neurotic groups were undertaken.

Chapter VII, *Experiments with Artificial Neuroses* (239-66), employed hypnotic suggestion to advocate or prohibit the use of certain words. Conflict was elicited when the experimenter instructed subjects to use contradictory sets of words. The responses were comparable to those suffering from compulsive neurosis. But conflicts could also be isolated or resolved by using the connecting sphere to seek verbal compromises. These experiments took place c. 1926-7 in collaboration with B. E. Varshava and V. I. Zatrezhnev.

Chapter VIII, *The Structure of Conflicting Processes* (267-300), followed up the different outcomes of the above experiments in delays to activity. Luria looked at the effect of displacement to the connecting sphere. He concluded that the likely explanation for this is that the receptory-connecting system and the effector system “play functionally unequal roles in the activity of the organism, and they control noncomparable structures” (289). This means that in the normal adult there are two phases to his or her activity and that there is a barrier between these two phases that obstructs the direct transfer of excitation to the motor area. This Luria explored in experiments with patients suffering from aphasia and Parkinson’s disease. Luria’s collaborator at the laboratory of the Clinic for Nervous Diseases at the First Moscow University was M. S. Lebedinskii (cf. Lebedinsky & Luria 1929). They investigated more than 200 patients there (ibid. 474). While this work dates from before 1929, some of the other experiments in section 3, for example, may have occurred a little later.

Chapter IX, *The Dynamic Analysis of Conflicting Processes* (301-28), uses some of the research collected in the previous chapter, though more probably from late 1929-1930, to make the structure of the reactive processes and their separate but linked parts more comprehensible. It introduces a revolutionary new approach (Luria 1932a, 302-3) which examines the structural and neurodynamic implications of looking at the ontogenetic development of levels of organization in a dialectical way. Luria concludes that “the inclusion of speech alters fundamentally the organizational principles of behaviour. It changes the natural forms of the gradual organization ‘from below’ to the cultured forms of behaviour ‘from above’” (303).

Part III, The Genesis of the Reactive Processes and the Psychophysiology of the Control of Behaviour, assesses the theories that Luria was developing. Control of behaviour was evidently possible only as a human matured and Luria sought to explain how this control became possible through the role of speech and other signs. He studied the acquisition of speech in child development, and its partial loss or disintegration in cases of aphasia, Parkinson’s disease, hysteria and neurosis. He used his theoretical explanations to help patients to develop forms of compensation for their illnesses. Luria wrote:

The complex forms of organization and disorganization of human behaviour can in no wise be explained as a simple play of neurophysiological processes, ... no phenomena of elementary neurodynamics can elucidate these configurations of integrated behaviour specific for the human being as a social subject. It is more probable that elementary neurodynamics, as observed in the human, is comprehensible only by an analysis of those higher forms of organized behaviour connected with the culturally created psychological functions as in, for example, the complex behaviour of work, speech and intricate indirect operations. The inclusion of neurodynamics in the system of such higher psychological functions brings about a specificity of its organization.

The desire to study the development of these higher forms of the regulators of human behaviour led initially to genetic experiments whose purpose was to investigate the regulation of behaviour in early childhood, and to experiments dealing with pathological material in which models of these regulations were

created in an experimental situation. Thus we were able to study the mechanism of the control of behaviour. Part III is devoted to this subject (Luria 1932a, x).

This was the culmination of the first phase of Luria's work and a platform on which he based the rest of his life's work. It could also be said to have laid the basis for an integrated psychology, and thereby helped to resolve the perennial 'crisis in psychology', which we will expand on later (VII, iii).

Chapter X, *The Development of the Reactive Processes* (331-66), reported on simple reaction experiments with children and adults, to determine the differences in their structures of reactions. The development of neurodynamic structures was now an intrinsic part of the whole approach. In this it began to follow up Luria's theoretical advance in Chapter IX. The experiments took place c. 1929-30 at the Institute of Experimental Psychology, and at the psychological laboratory at the Communist Academy of Education and the Clinic for Nervous Diseases. Although Luria had other unnamed collaborators, most of the research used was that undertaken by M. S. Lebedinskii. Statistics and other relevant material not published in Luria's book are to be found in the book by Lebedinskii (Lebedinskii 1931). Luria also used material collected by P. S. Lubimov, a colleague at the Institute.

Chapter XI, *Nature of the Functional Barrier* (367-96), began to address the significance of Henry Head's work on aphasia. (At the beginning of the chapter I interpolate a review of relevant ideas from Head, Hughlings Jackson and Anokhin). It looked at experiments discussed in Chapters VIII and IX, including the independent work of Lebedinskii and Lubimov. It also used experiments from c.1929-30 comparing those suffering from functional neuroses, the learning disabled, and those suffering with fatigue to clarify the nature of the 'functional barrier' and demonstrate its development as *part* of higher psychological functional systems. These latter, when taken as a whole, are in a sense as much the subject of this chapter as the titular part. An attempt to draw out some of the elements that deserve amplification occurs later (cf. VII, iii).

Chapter XII, *The Control of Behaviour* (397-428), reported on experiments undertaken in c.1929-30 with children, hysterics, and sufferers from Parkinson's disease. These concentrated on showing how indirect stimulation, including auto-stimulation is central to higher psychological processes and enables an individual to control his or her behaviour and is an important approach used in compensating for weaknesses.

In his preface Luria is clear about the changes he has made:

In this latter series of experiments the author attempts to define his overall psychological point of view. Whilst dealing with the experimentally manifested psychophysiological mechanisms of affects, complexes, and conflicts, he does not become a psychoanalyst; nor a behaviourist in objectively analysing the psychophysiological structure of the disintegration and integration of the psychical apparatus; and least of all does he attempt to deduce the laws of higher activity from simple neurodynamic processes.

The author does not believe that the problems of the most complicated forms of human behaviour can be solved by the laws of the dynamics of tendency, nor by the analysis of the conditional reflex connections playing a role in the nervous system. The solution of this problem will be attained only by a careful description of the specific problems of behaviour produced in the process of socio-historical development, which are distinguished by the peculiarities of the human, and without which the organization of the higher neurodynamics remains incomprehensible (Luria 1932a, x-xi).

Below I have attempted to give an account of key theoretical developments to be found in *The Nature of Human Conflicts*. I have done so on a chapter by chapter basis, and placed the chapters in bold type when moving to a new chapter. Consequently, as stated in the preface, the references in this chapter and the following one usually comprise only the page number. This provides a fairly comprehensive review, but one in which I try to draw the threads of Luria's theoretical development together. I have attempted to use Luria's own words. My comments, elaborations and clarifications are, I believe, relatively easy to differentiate from Luria's text and my summaries of it.

Although I have made many amendments to Gantt's translation, these are usually consistent with his translations and, I trust, the Russian original. They are intended to remove confusing formulations and sentence structures, and thus to make it clearer and more consistent with what I understand as English. Without the original Russian it is impossible to convey Luria's style. In it, sentences are lengthy, clauses are many, and translations can only work if they are elegant. Gantt's translation does not permit a reconstruction of such a style. I have also regularly interpolated words in square brackets to help clarify the meaning. It is as though Gantt made an initial translation and then exhaustion prevented him making a consistent attempt at making it rational or readable. It is willy-nilly diversionary and barely digestible, which is why I have concentrated on drawing Luria's threads together. Having said that, while many page references refer to passages that may be hard to recognize from my reorganizations and 'retranslations', there are many lucid passages that I have left unchanged or virtually unchanged. Also I have selected excerpts, and made corresponding alterations and interpolations that have nothing to do with my criticisms of the text.

My own struggle with Gantt's translation has itself been relatively exhausting. However, it is not simply a question of readability and comprehension. There are clearly *many* occasions when Gantt, though then at the forefront of Western knowledge about Russian psychology, simply fails to grasp Luria's intentions. A comparison with my version of his translations will show this. Without the Russian original, it has proved extremely frustrating. Although at times it may prove that I have re-interpreted too freely, at others I have felt obliged to leave certain unsatisfactory passages untouched or even unused, rather than risk adding to the confusion. Similarly, terminology will have changed over the last seventy years. Ideally I should have consulted a neuropsychologist about many terms, but I felt it appropriate to wait until the publication of the Russian original. I have however usually replaced 'adequate' by 'appropriate', 'destruction' by 'disintegration', 'intellectual' by 'higher cognitive' or 'higher psychological', and 'psychoneurology' (which Gantt translates as 'psychobiology') with 'neuropsychology'. In the last instance, it should be noted that the Russians

considered psychology to be the dominant partner in neuropsychology (Rawles 2002, personal communication). I have inevitably made other alterations, but since there is no way that I can yet satisfy myself, other readers will also be dissatisfied with my omissions and compromises. The intention is clear, however, namely to make Luria's formulations more comprehensible than hitherto. This must also be recognized as an ongoing process. The need for this was expressed clearly and often at considerable length by the book's early reviewers (cf. VII, iii, 212).

II

The Introduction

The Introduction, i.e., **Chapter I**, offers a major challenge to the claims of the old theories and presents qualitatively new approaches. Therefore it is a chapter one cannot afford to skip. Consequently, I have quoted extensively from parts of it all through this section. Luria comments on this new approach throughout the work, and the theory is substantially extended in Part III. The chapter opens with the words, "We are concerned here with the investigation of the disorganization of human behaviour, with the mechanism of its falling and rising" (Luria 1932a, 3). Whether or not this is an oblique reference to Hughlings Jackson's famous work, *The Evolution and Dissolution of the Nervous System*, it nevertheless clearly intends to cover a similarly wide area. Luria notes that [contrary to Jackson's position] it is common to see the whole nervous apparatus as consisting of separate neurons, and the brain as "nothing more than a centralization of them and their circuits... Consequently the laws of behaviour must inevitably be influenced in those laws which already hold for individual neurons. The whole of behaviour might be understood merely as the preservation of equilibrium between the separate apparatuses of the nervous system. Its pathology is understood as destruction of this equilibrium" (5). Since "the elementary processes of excitation and inhibition are the basic ones which are found in every nerve cell, they are carried throughout the whole organism, and the most eminent school of objective neuropsychology attempts to explain every process of behaviour in terms of excitation and inhibition... The normal behaviour of the human is consequently examined as the preservation of a certain equilibrium between the inhibitory and

excitatory processes". Luria then refers to the works of Pavlov as examples of this (ibid.).

In respect of the organization or disorganization of human behaviour and the disturbances treated by neuropsychology this approach was not simply partial.

Researches in human behaviour, both normal and pathological, lead us to doubt the adequacy of these fundamental conceptions. The facts that we observed demonstrate convincingly that behaviour cannot be explained as an equilibrium of separate systems, and that the concepts of elementary inhibition and excitation, not being included in the highest and specific whole [but only at an elementary level-MH], are completely inadequate... [Disease of the mechanisms involved] does not by any means evoke the same affect and often causes general changes, which are comprehensible only [within the context] of a most complicated functional reciprocity of internal behaviour (5-6).

This inadequacy is manifest in the case of aphasia, the disturbance of the function of speech and symbolic activity, where the most marked feature is confusion.

Can we understand [the symptoms of aphasia] only as a failure of a separate system, or can it be expressed in terms of inhibition and excitation? ... Attempting to express this state as a complicated mosaic of inhibition and excitation does not satisfy us in any way. Undoubtedly both neurodynamic forces, inhibition and excitation, are included in our [experimental] phenomena, but it would be naïve to represent or presuppose that they are created from those elementary processes... Inhibition and excitation are included here in a higher complex whole, and may be understood only on this basis. Entering into the whole, they inevitably acquire a qualitatively new significance, inhibiting as well as organizing the role of speech. (6).

The structure of the organism presupposes not an accidental mosaic, but a complex organization of separate systems. This organization is expressed most significantly in the functional correlation of these systems, in that they do not combine together in an accidental way, but unite as very definite parts into an integrated functional structure.

The basic feature of this overall structural organization of behaviour is a functional inequality of the different systems entering into it. Certain systems appear as governing and regulating, others as subordinate, executing one function or another. It is clear that the significance of these in the system of organization is not always the same. The whole activity of the organism can be understood only as a dynamic system, a conditioned activity of its component parts. It is hard to describe this system in terms of inhibition and excitation, but much more appropriate here to consider the conception of organization and disorganization, which regulates the disintegration of the system of behaviour. In these concepts we see a far greater possibility of understanding the dynamics of behaviour than if we approach the subject from those mechanical concepts described above.

The behaviour of the aphasic person would be more comprehensible to us if we attempted to depict him in terms of organization and disorganization. ... The cortex, and in particular its highest parts, has been for a long time described as having a regulating function. This function was discussed in a series of special investigations on speech and symbolic activity... Speech and the higher psychological processes that played a special, regulating and leading role in behaviour were thus distinguished from other processes. It is understandable that an injury to these higher processes should produce not a partial destruction of definite processes, but the destruction of the whole system of behaviour that is shown to be incapable of functioning with a destroyed executive regulating system. The confusion observed in the behaviour of the aphasic person, the uncertainty of all his activity, the disorganization of his behaviour, which has been so brilliantly described by [Henry] Head [in *Aphasia and Kindred Disorders of Speech*], is comprehensible if we direct our attention to the neurophysiological apparatus as a system of separate partial apparatuses capable of being inhibited or stimulated. We shall proceed from this conception of the organization of behaviour connected with the distribution of the executive regulating systems.

The mechanisms of inhibition and excitation serve us admirably to understand the processes of regulation and disintegration, but they participate as part of the mechanisms [undertaking] the general purpose, and will be best understood only in the light of the complicated dynamic relation of the separate systems of the organism. It is thus the organization and disorganization of human behaviour, and the conditions, laws and forms that appear as the most important problem of neuropsychology... (6-8).

Thus Luria wrote in the opening pages of his work. Today someone unaware of the history of the subject might accept Luria's words as fair comment and proceed unaware of the enormous change in thinking that Luria had been part of in the nineteen-twenties. I did not fully appreciate the significance of Luria's achievement myself till I had completed the previous five chapters. But while Luria has evidently and conclusively subsumed various reductionist concerns as real yet subordinate – as well as lower – elements of human behaviour, he had also differentiated his ideas from those of Goldstein and Lewin. Their emphasis on wholes and schemata, though crucial in breaking with the old reductionism, did not embrace the concepts of an interrelationship of many unequal systems or the role of speech and other symbolic systems in the restructuring of consciousness. But there was still much in common between Luria and the 'structuralists', as they were known in Russia.

Luria continues: "The conception of structure and organization does actually make up most of the new neuropsychology, and based on these concepts are the latest ideas expressed by Koehler, Koffka, Wertheimer, Goldstein, Lashley, Child and others..." But there were dangers in some of these approaches, such as "the wish to transform the complicated forms of the organization of behaviour into general laws..." (8).

This "inevitably leads us to ignore and misunderstand the details of human neurodynamics and the highest and specific forms of behaviour always remain beyond the field of vision of the mechanists. The opposite danger is represented by those who connect the principles of behaviour with a vital structure". Even the best minds, such as Constantin von Monakow see "the higher forms of organization as the products of some special forces, [and thereby] exclude every possibility of a scientific investigation of the mechanism of this organism and replace analysis by postulating some new entity, obscure and not accessible to analysis" (9).

Luria could not ignore general laws or special factors, but he could not accept simplistic formulations. One approach in particular seemed to demand answers that could not be satisfied within these limits:

In the first stages of this development the forms of the organization of behaviour are certainly of a different order than those forms of organization which differentiate more complex behaviour. And this development proceeds more along the path of dominating the primitive laws, than along the path where they are simply repeated in the new stages. The problem of human behaviour proves, we think, to be the problem of development, and only this way can we reach an understanding of the mechanism that lies at the basis of the activity of the human personality (*ibid.*). [Luria elaborates on these themes in Part III of the book].

He continues:

The material makes us think that the genesis of organized human behaviour is through the development and inclusion of all the new regulating systems, which overcome the primitive forms of behaviour and transfer them to that which is a new and more systematized organization. There is every reason to suppose that the primitive forms of the organization of behaviour, characterized by the sub-cortical type of activity, are completely transformed into the processes of the highest development... This replacement of one type of behaviour with another is connected with the development of newly regulated systems, coming into conflict with the primitive sub-cortical activity and overcoming it, creating all the new forms of organization (9-10). These new forms of organization are not in any way organized – as many authors think – by the development of inhibition and the restraining influence of the cortex on sub-cortical activity. Neurodynamic development from early childhood to adulthood results in a gradual overcoming of primitive diffusion in the activity of the nervous system and the elaboration of new functionally organized forms of behaviour. In this process, the higher cortical mechanism does much more than play a simple negative role. Precisely because of its participation in behaviour, those regulating systems [can] transfer the organization of behaviour to higher and higher stages and create totally new forms” of regulation and behaviour...

The inclusion of [higher psychological mechanisms] in the behaviour of the child began first with the complicated organic mechanism and then with the higher cultural systems, which condition new forms of organization (10).

This approach dispenses completely with both abstract general laws and any need for 'vital forces'. In a completely concrete analysis of the organization of behaviour, Instead "we treat it as a function of definite regulating systems, unequal at various stages of the development of behaviour, and fully accessible through scientific analysis" (ibid.).

This is examined in the investigation of the disorganization of behaviour. Many writers have avoided going beyond description in this field, but even when scientific developments promised the possibility of objective study of it, many still balked at the idea or explained it as a loss of equilibrium. Disorders that were not physiological were also often treated similarly – as in the James-Lange theory of emotions. [This theory held that "emotions were caused by changes in the voluntary motor sphere and by involuntary changes in the visceral sphere" (Petrovsky & Yaroshevsky 1987, 158)]. Indeed "the present investigator, in attempting to work in this field, finds that he is building on air or, in any case, on very uncertain ground" (Luria 1932a, 11). However, the research of W. B. Cannon and Pavlov was important, and several others had tried "to introduce affect into the system of active human behaviour... Kurt Lewin attempted to show, in a series of carefully executed experiments, a more sharply marked relation between the processes of tension, discharge and affection" (13).

Many scientists, however, measured excitement itself rather than seeking to find measures that could examine which particular mechanisms and structures were involved, let alone the dynamic interrelationships of these structures. "In order to do this, we study the symptoms, mechanisms and dynamics of affect as one of the existing forms of disorganization of human behaviour. We try especially to think of the conditions of the origin of this disorganization, and of those systems that play decisive roles. We also apply the methodology of psychology to physiological processes, not forgetting for a minute that we are studying the structure and function of human behaviour" (13-4).

In the second, larger section of the Introduction (14-39) Luria considers various investigatory approaches. The subjective school had never progressed beyond the stage of describing symptoms. Wundt's theory in effect concluded that "affect is not an act of behaviour but a reaction to a series of psychological problems". This resulted in "the peripheral theory of emotion" [as in James's approach –MH], which in turn also led to the investigation of symptoms, but no explanation of the processes involved (15). What was important was a way of measuring behaviour before, during, and sometimes after the period of affect. How else could one measure the changes in structural dynamics? It was necessary to turn from "the observation of peripheral symptoms... to the study of the structure of the central processes...". This would be in keeping with the findings of Cannon, whose animal subjects continued to show affective behaviour after the excision of the viscera. "These experiments confirm that the physiological changes usually considered as the basis for the affective processes are in reality only secondary and accompanying processes, the diminution of which does not remove the affect". The peripheral approach was therefore inappropriate. "Only in the alterations of the active forms of human activity can we hope to find a suitable reflection of that structure of the affective processes in which we are interested... The affect appears when something happens with the organized phenomena of activity. Therefore it should be reasonable to hope to obtain a more adequate structure of the affective processes by the investigation of the fate of the active functions connected with this process" (17).

The second consideration follows immediately from the first: only a system of active behaviour – speech or motor – appears capable of manifesting an actual structure that changes under the influence of affective behaviour. All the events of interest to us that cannot be expressed in physiological symptoms are fully obtained in the structure of active 'spontaneous' acts.

We may follow at a glance the variation and conflict of the experimental character, whether active or passive, of the reaction, the disturbance of the process, and its control. The complicated character of the behaviour, directed to a known external activity, allows us to estimate with great exactitude not only the general character

of the disturbance. And also it allows us to ascertain in which system of activity, in which of its phases, from the very beginning to motor termination, arose those changes that elicited the characteristic disorganization of behaviour. Consequently this enables us to turn in a new direction in the investigation of the affective processes, replacing the study of symptoms by the investigation of the structure, switching from the path of physiology to that of psychology (18).

Furthermore not only could Luria amplify the disorganization of behaviour in respect of the central processes through the use of activity in the verbal and motor systems, he was also able through these very same systems “to reflect this process in systems accessible and suitable for examination. The motor function is such a systematic, objectively reflected structure of the neurodynamic processes concealed from immediate examination. The use of the motor function as a system of the reflected structure of hidden psychological processes is therefore available to us. We proceed therefore along the path that we call the combined motor [and verbal response] method” (ibid.).

We must find a system of activity such as will include in [both] its parts and central process the affective disorganization involved, and the motor process that should be capable of reflecting the central activity and its fate. This motor process should not be something extraneous, but a special phase [of the activity], included within the overall structure. Only under these conditions of the participation of the central changes and motor-reflected processes in one general structure can we hope to adequately represent in our study all the phenomena arising in the concealed concatenation of changes. Certainly if we combine in one functional system two activities, the central and the motor, we can record that every central change is of necessity primarily reflected in the motor system. The latter is formed into a united whole [in the course of action], and only secondarily evokes certain changes in the physiological system to which it spreads. Such a division of the united dynamic structure, including the central part concealed from direct study and the motor functions capable of being objectively registered, is the basic combination of the motor method with which we have acquired the essential material dealt with in this volume (22-3).

Thus we have revisited the point where we introduced the combined verbal and motor response method in the discussion of Luria's experiments (cf. I, iii and particularly II, ii). In retrospect we can clearly see how the methodological 'time bomb' implicit in this method, as described by Radzikhovskii and Khomskaia, at last emerges. This is acknowledged implicitly in the fact that, at this point, Luria cites two of his papers on the method (Luria 1928c, 1929a). Equally significant, or perhaps more so, is Luria's above reference to his version of the functional system, again explicitly in connection with his method. In this case the practical method had eventually led to the introduction of "a psychological theory capable of assimilating the data obtained by Luria" (Radzikhovskii and Khomskaia 1981, 8-9; cf. the discussion of this in II, ii, 55-6).

Luria now takes up the speech part of his 'prototypical' functional system.

The subject must reply to a word that is given him with the first thought that enters his mind verbally. At the same time he must press the finger of the right hand on the receiver of an apparatus lying before him. Here we stimulate in our subject two systems of activity that are connected with each other so closely that they are set in motion by two simultaneously occurring activities of one and the same process. Actually, the proposal to answer a given word by any other word excites in our subject a certain central process of a very complicated order, one close to the speech system. Analysing it psychologically, we can see its associative process in some cases; in others, its primitive fate... (Luria 1932a, 23).

We are concerned here not with the phenomenal existence of this process. Our attention is chiefly directed to the fact that we are able to evoke a definite, very complicated neurodynamic process that is concealed from immediate observation, which, after a certain period, leads to a verbal response. This neurodynamic process can be at one moment entirely organic and regular, at another moment it may meet a certain obstacle in its path and collide with it, resulting in a certain disorganization. It is clear that the neurodynamic process that lies at the base of the habitual associative response is actually different from that characteristic of the higher psychological process. The former varies in response to, and is obstructed by, the affective tone... In all these cases the structure of the neurodynamic processes will of course be very different and appear inaccessible to direct and

objective analysis. Our concern consists in attempting to experimentally bring the disintegration to the structure and by this approach permit the emphasis to be placed on the analysis...

In uniting the word response and the motor reaction into a single process we have a method by which we can estimate the actual changes in this obscure process as necessarily reflected in a clearly defined process, whereby we see the differences in the neurodynamic structure of the central process reflected in the evident differences of structure in the motor curve. Precisely this union of both functions into a single active system leads us to believe that every sharp fluctuation and every tendency to a speech response and, even more so, every marked affective disorganized character of the central process affects the structure of the combined motor reaction. In analysing this, we [now] have available a very objective means for drawing conclusions concerning the structure of the internal dynamic process (24).

We shall continue discussion of the overall theory in the following chapter.

The remainder of the Introduction explains in detail how the combined verbal and motor response method is to be applied and interpreted. There are several photographs showing the apparatus in use and figures showing examples of experimental responses, as there are throughout the book. Figure 5 (27) is an example of the possible reactions that could be registered. Since we have discussed the method earlier (I, iii, II, ii) we will not elaborate further at this point. It should be noted that the 'Luria technique', as it came to be known, received a great deal of favourable attention at the time and was adopted in Western experiments. Luria's claims for its validity were generally accepted and, while Luria remained working in psychology and in communication with the West, i.e., until 1936, his reputation remained high. The literature on this is quite extensive (cf. the references in Reymert and Speer 1938-9). His subsequent use of similar techniques in measuring the control of behaviour in developing children was also favourably received (cf. Beiswanger 1968, Jarvis 1968, Wozniak 1972, Wilder 1976, Bain 1976, Pressley 1979). It was possible to simultaneously record the subjects' verbal responses, but not practical to display them on the same sheets that gave the motor reactions. Today this would be relatively easy to do. A

comparison of the use of the 'Luria technique' in an experiment along his lines with a simultaneous brain scan would probably be a more appropriate contemporary method of confirming his findings, though this does not appear to have been undertaken.

III

Part I

We have already looked at the first experiment in Part I – that which took place at Moscow University in the spring of 1924 (cf. II, ii). **Chapter II** also deals with a similar set of experiments that took place in the autumn of 1927, again in collaboration with A. N. Leont'ev. Here 51 men and 58 women aged from 18 to 35 took part in an experiment prior to and after exams. Similar word responses were measured. Before the examination 72 per cent showed signs of disturbance, 11 per cent markedly so. "Both of these figures coincide almost exactly with the results obtained" in 1924 (Luria 1932a, 65). Disturbance or breaking of the regulatory restraint was also evident, sometimes "destroying that which we must conditionally designate as the 'functional barrier'" (72).

But in addition to Luria's introduction of this term, which he examined at length in Chapter XI, he also compared the responses of psychological groups, some of which were considered 'stable' and others 'labile', a terminology borrowed from Pavlov. Luria had medical investigations carried out on both groups. The results showed some normal reactions among the labile group and vice versa. Luria concluded, however, that "the degree and character of the reactions of the individual to the affective situation are primarily connected with the neuropathic status, with that fatigue or weakness of the nervous system in many of our 'workers by brain' [i.e., 'white collar' workers]" (74-5). "Here we witness the confluence of affect and neurosis. The affective situation provokes a reaction similar in structure to that of neurosis. It creates, as it were, a temporary but real neurosis, which is most distinct in those subjects already showing a neuropathic disposition". Luria concluded, on the basis of later findings about his subjects, "that our research may serve as an early diagnosis of neuropathic disorders" (75).

Indeed he was to follow up these ideas in Parts II and III of his book (cf. also Clarke 1955).

Since we have already looked at the contents of **Chapter III** on affect in suspect criminals (cf. IV, iii), we will turn directly to the subject of **Chapter IV**, 'The Investigation of Complexes Produced during Hypnosis by Suggestion'. (This is the main area of new research to be introduced in this section). Hypnotism, incidentally, remained a subject of interest for Luria in the 1930s. An article by K.I. Platonov, *On the Objective Proof of the Experimental Age Regression*, received by an American journal in August 1932 was accompanied by a recommendation from Luria. Platonov notes intriguingly (Platonow 1933, 208), "At present we are starting, with Professor A. R. Luria and Dr. M. S. Lebedinskii, a series of investigations which ought to provide more objective proofs in support of the reality of the phenomena of regression to previous ages". Unfortunately I have found no publication on this listed in the works of any of the three investigators. One possible reason is the personal attacks on Luria that occurred at this time. More likely it was due to government policy of cracking down on 'cosmopolitan' and 'degenerate Western' concerns. This would explain why neither the topic nor the above publication is mentioned in Platonov's major English publication (Platonov 1959), even though hypnotism itself is discussed positively, having been sanctioned by Pavlov's use of it.

This chapter, however, deals with research that was mainly carried out in the period 1924-5. As we saw in the introduction, several doctors were involved in ensuring that the subjects' health was not put at risk. Luria's principal collaborator was Boris Efimovich Varshava (1900-1927), who was also a member of the Russian Psychoanalytic Society. A tribute to him by Vygotsky can be found at the beginning of the psychological dictionary Varshava was compiling and which Vygotsky completed (Varshava & Vygotskii 1931).

Luria begins by noting that in 'real-life' experiments, such as those that he had undertaken with criminal suspects, one could never eliminate the possibility that behavioural disturbances had other origins. "The ideal for the experimenter in psychology has become the option of artificially reconstructing the phenomenon

under examination, because this enables one to keep it entirely under control... It is therefore quite comprehensible that only the artificial insertion of an affective complex, known in all details, into the psyche of a subject, can create a situation for the psychologist in which it would be easier for him to record all the factors forming the affective reaction” (Luria 1932a, 129).

We suggested to the subject, while in a sufficiently deep hypnotic state, a certain situation, more often a disagreeable one, in which he played a role irreconcilable with his habits and contrary to his normal behaviour. We made those suggestions insistently and forced the hypnotized person to feel the [conflict-] situation with ‘appropriate’ pain. We thus obtained a real and rather sharply expressed acute affect. We woke the subject and allowed him a period of amnesia, natural or hypnotically suggested. We thus had a subject ‘primed’ with certain definite affective complexes, mostly unknown to him, but recorded by us in almost all important details (ibid.).

Given the period of these experiments, the experimental design, and the non-conscious suggestive elements involved at the core of these experiments, this can hardly be other than the basis of the article that Kornilov refused to publish (cf. V, iv, 141-2). Indeed the title of the unpublished article, *An Experiment in Objective Psychoanalysis*, is echoed when Luria refers to “such ‘experimental psychoanalysis’” (Luria 1932a, 150). Here, though, it is necessary to note that Luria placed the term in inverted commas, i.e., while the experiments might still be relevant to psychoanalysis, he himself saw it within a new framework.

“The suggestions that were given ... were usually received with active objections...” (131). So traumatic was the process that subjects were carefully screened, prepared, and attended by a physician during the experiment. Subjects were observed making “restless defensive movements, trembling during sleep”. In one example, K, a 23-year-old obstetrics student is told she is in a position where she is being offered money to perform an abortion on a desperate woman. K has no right to perform this and vigorously refuses. The experimenter is only able to bypass the subject’s objections by stating, “You have agreed and the woman has

gone away". The subject is then woken, and asked how she feels. "Something very disagreeable has happened, but she does not know what" (132).

"Suggesting a conflict connected with *action*, we obtained an affective reaction. This is one of the fundamental ideas of this book", and distinguishes it from previous experiments, such as the school of Wundt, which used agreeable and disagreeable stimuli to produce 'evoked emotions'.

We have an entirely new structure of the affective process, compared to those previously analysed. Here a strong emotion is hidden in the past and concealed not only from the experimenter [at this stage of the experiment], but also from the individual herself. It is removed from consciousness, though apparently still active. It is quite natural that we should expect symptoms entirely different from those which we observed in cases of marked and violent emotion (133).

The results from three sets of tests using selected word stimuli were compared. The first took place before the introduction of the hypnotic suggestion to the subject, the second took place when the subject was under its influence, and the third took place after the suggestion had been removed. Luria was able to draw several conclusions from the information gathered:

We have before us artificially obtained symptoms entirely similar to those which we observed in cases of natural affective traces. The difference lies only in this: three varying situations, which are usually observed in different subjects, are here shown in three tests with the same subject. The first test deals with a man who is in a state of emotional balance, whose behaviour is entirely organized. In the second, a personality with a markedly apparent central defect, similar to the one we observed with criminals or with hysterical people. The third one shows again a picture of more or less quiet state with but few traces of these past experiences or of reactive, sometimes completely eliminated affect (136, 139).

The second group of tests proved particularly interesting. In some instances the affective symptoms had a distinctly localized character.

We believe the concentrated nature of the traces, not passing into irradiated disturbance of behaviour, is in this instance a result of the disconnection of the affective complex from consciousness... [Here related] with separation from the motor area...

We encountered affect every time when some initial activity was retarded and the potentially powerful activity of the organism was inhibited... There are two ways of checking it – either by providing an outlet for the tension which accumulated as a result of the retardation, or by removing it from the motor area. We have seen how the first way appears in the act of admission (149) [cf. the confessions of criminal suspects].

The second way involves the post-hypnotic act of forgetting the complex.

[This is a] question of vast importance to psychology, as well as in the pathology of psychological situations. The amnesia that we created artificially, insulating consciousness from the traumatic picture, leads to a process quite similar to that of displacement. [NB: Gantt translates this as ‘shifting’ –MH]. The authors who described the latter, beginning with Freud, have often pointed out that the active side of the insulation from consciousness must be seen as insulation from the motor area... The connecting of consciousness is one of the most interesting problems of psychology... [It is summarized in the formula:] Removal from consciousness: insulation from the motor area; consciousness: spread into the motor area (149-50).

Our tests make it possible to observe what neurodynamic factors are being evoked by consciousness of the suggested affect. The associative experiments [of selected word stimuli] that we used seem especially well adapted for this. Each stimulus connected with the suggested situation provokes distinct disturbances in the reactive process. Every such stimulus is, however, another step toward breaking the barrier that separates the affect, already introduced into the psyche, from consciousness... By using a prolonged associative experiment we may ... build a model of the very process of its becoming conscious... It then makes it possible to trace the neurodynamic mechanisms connected with that consciousness... We may ... construct a much fuller picture of such ‘experimental psychoanalysis’ if we adopt prolonged and more adequate observations (150).

In this respect Luria refers again to the case of K, the obstetrics student.

The object of our test is to trace the *neurodynamic correlations of that gradual [attainment of] consciousness*, and thereby clear up the problem of the psychophysiology of the conscious [as distinct from] the inhibited affect... After having constructed an 'experimental unconsciousness' we would naturally employ, as a method, 'experimental psychoanalysis'. Free association was the best means for our purpose. Being determined not only by the logical, but also by the affective data of the personality, this method clearly showed the inhibited complexes. The acute character of the suggested affective complex, ensured that it would be exposed by even a small number of free associations (151).

Luria seems to be making something of a joke in his references to psychoanalysis, but quite a complex one. He is probably referring to the time of the original experiments when he was a major figure in the psychoanalytic movement in Russia. But there was an element of ambivalence then in that, unlike other psychoanalysts, he was always involved in 'experimental' and 'objective' research. By the time he was writing this up, he was no longer a member of that school. But here he is also commenting on the censorious views of the Russian rulers, or rather their 'intellectual' agents, towards psychoanalysis. The quotation marks imply his distance from *real* psychoanalysis, but he seems to be saying also that he, as a scientist, has the right to investigate and utilize the methods that he considers to be appropriate. One wonders what subtleties may be revealed in the Russian text, though the following passage is also intriguing. An ironical reading could explain why Luria gained a reputation among his friends as something of a joker, though there may be no irony intended.

In one important respect the method of our free associations was distinctly different from the psychoanalytic method of research. It was contained entirely within the framework of a psychophysiological experiment, and herein lie both its strong and weak points. The complex, the appearance of which we have studied, was known to us beforehand, and this is what made it possible for us to avoid complicated methods and interpretations. We had no ground to believe that the [hypnotic] suggestion would be subjected to some especially deep modification, and we expected that it would appear almost straight away in the first or second association

series. Noting all associations that ‘freely came to mind’, the intervals between them and their connected symptoms, we were able to obtain accurately fixed associative series, and to study in detail their dynamics (151-2).

[Naturally the experimental approach] entirely excluded extensive research on the problems of the unconscious and of its dynamics. This was not, however, the object of our psychophysiological work (152).

It should be noted that Luria’s comment on the ‘associative series’, also known as the ‘(free) chain association’, refers to a 1927 work by A. N. Leont’ev titled *The Structural Analysis of Chain Associative Series*, which suggests a further refinement in the approach that is related to the sequencing and intervals of the word stimuli – for example, bunching of words likely to produce an affective reaction.

In the case of K – or Kar – as she has now become, her motor reactions both before the hypnotic suggestion and after awakening from it proved regular and normal in the first test and initially so in the second. Luria’s preliminary conclusions were as follows. “*The insulation of affective traces from consciousness simultaneously produces insulation from the motor area, transforming active affect into one that is concealed or potential*” (154). “However, the appearance of irregular intervals at the end of the chain series [in a later part of the test], and the presence of some single marked inhibitions is of special interest to us and leads us to suppose that some destructive factor has intervened”. These associations are understood by the experimenter, but not yet by the subject. This shows convincingly *that important symptoms of neurodynamic changes are connected with reconstruction of parts of the complexes in speech series*” (155). “The later symptoms were more intense. *There is a clear appearance of the affective situation in the subject’s consciousness, followed by a motor storm, which, for a time, breaks up any normal reactive process*” (156). “The dynamics of the suggested complex now become sufficiently clear. Being disconnected from the conscious area, that complex, nevertheless, shows an insistent tendency to creep into the speech series... *The affective complex constructed by us, though not yet conscious, creates an affective state and*

determines the flow of the free associative series" (157). "The connection between the process of becoming conscious and the removal of the reaction to the complexes – the basis of psychoanalytic therapy – becomes much clearer. In fact, the end of the series quoted above - [the text contains both the word series and the motor reaction graphs – MH] – shows with sufficient clearness that after the basic complex has become conscious, the subject is able to pass into a considerably more stable chain of neutral reactions than previously" (159).

On the other hand, the neurodynamic explosion, which appears [initially] after the affective complex has become conscious, presents a new view of the mechanical structure of displacement. The insulation from consciousness and the simultaneous insulation from the motor area seem to be the mechanism that *saves the personality from the over-excitement and disorganization connected with an open appearance of conflict*. In that respect the construction of a certain functional barrier between the affective centre and the motor area is of decisive importance for the personality in retaining the possibility of acting normally... (159-60).

Luria then goes on to discuss a case where the suggestion, though very traumatic, was not accepted during hypnotism. This produced quite different neurodynamic symptoms (161 ff.). He concludes, "Substantial confirmation of the differences between the psychophysiological structures of the complex and the trauma are found in the different pictures of hysteria and traumatic neuroses", particularly with respect to the involvement of the motor area (167).

Luria's investigation of the structure and dynamics of affective complexes and their route to consciousness seems in retrospect a remarkable series of experiments, worthy of far more attention than the first two series which we discussed. Strangely, it seems to have been largely ignored, even by those psychoanalysts who ought to have been interested in what seemed consistent with, and very relevant to, their approach (though cf. VII, iii, 212). On the other hand it is clear that Luria had no difficulty in discussing the processes discussed above in terms that were not specifically psychoanalytic, consistent with his own theoretical development.

Chapter V, ‘The General Features and Mechanics of the Affective Process’, provides a theoretical conclusion to Part I. Appropriately it starts by reminding us that “every investigation proceeds in cycles, and the attempt to complete one cycle is at the same time an attempt to begin planning around the issues of subsequent researches” (169). It is relevant, not only because it connects the experiments involving hypnotism in Parts I and II, but also because this chapter considers both the analytical and the synthetical sides of the investigation of the mechanics of affective processes.

Specifically, Luria notes that “the affective process is by no means equally related to all the stages of human activity, but it is connected with special functional zones, and [this connection] itself requires analysis” (170).

[While the majority of authors, including those of Wundt’s school,] were inclined to see in the affective process a functional connection with a special stable system ... we take the opposite position. In the unstable affective symptoms revealed we see a result of this, that *the affect is each time a function of a dissimilar structure*, that the symptoms of disintegration we study are parts of dissimilar units. We should [therefore] seek the neurodynamical laws of affect on the basis of a conception that takes account of these dynamic peculiarities (171).

Even when a person is in a more or less acute affective state, the different motor systems reveal the affect in unequal degrees. [But this is not simply a question of morphology]. We believe that the degree of expressiveness of this or that system *depends not so much on its anatomical position as upon its inclusion in one or another complicated psychological structure*. Therefore one and the same motor system can be either expressive or inexpressive, depending on what function it is fulfilling at the given moment and to what psychological structure it belongs. If, for example, the foot system is ordinarily the least expressive, then this is because it is ordinarily the least connected with the higher cortical processes and the least included in those psychological structures which have the property of maximum lability and are of maximally conflicting character (172-3).

“Consequently, the expressiveness of the system is conditioned not by its morphological but by its *functional* situation” (173). More specifically, “affective

disorganization of behaviour begins where the problem of cortical control by the direct diffusion of excitation arises, i.e., motor impulsiveness. It disappears where the action permits the direct motor discharge of impulses. *Affect* arises in the place where the conflict begins to be connected with activity” (174). At this point we will leave Chapter V and Part I. As we can see Luria was by no means in need of questions to ask, but which to deal with first, and how. As he noted with regard to his very first experiments involving fatigue, disorganization was intimately connected with speech and the manual motor area. The ‘Luria method’ was also intimately involved with this, as was the recognition that this involved a ‘functional system’ as distinct from a morphological system. That this system connected two initially separate systems led Luria to the point of recognizing the importance of such systems not only for the purpose of explaining affect and its role in the disorganization of behaviour, but of their importance for a great deal of human organization, activity and behaviour. *The Nature of Human Conflicts*, as Luria well knew, was eventually to have many implications for all other aspects of behaviour.

Chapter 7: 'The Nature of Human Conflicts' Parts II & III, and the Significance of the Development of the Concept of Functional Systems for Psychology

I: Part II

Part II, titled 'The Psychophysiology of Conflicting Processes', represents a later cycle of Luria's experiments. He differentiates it from that discussed in Part I at the beginning of the first chapter, **Chapter VI**, ('Experiments with Artificial Conflicts'). The new aim was to artificially create "a model of affective disorganization [without using] natural emotional tendencies, but producing it experimentally with psychological mechanisms that in themselves are not [directly] connected with any affect or emotion" (Luria 1932a, 206). "We will artificially introduce specific and completely isolated conflicts into the activity we are studying, and attempt to show under what conditions these lead to a widespread disorganization of human behaviour, to a model of artificial affect and experimental neuroses. With such an artificially created model we are able to control and understand the processes we have studied" (ibid.).

Luria's models were those of the experiments of Pavlov and his school (cf. Iv, ii, 107) and of Lewin and his school, to both of which he pays tribute. He notes, "here the fundamental conception of Lewin [i.e., to achieve the above] is very close to ours" (207). Rather than focus on introducing emotional disturbances that might spill over into the real life of the subject, the conflicts were introduced into what Luria called 'the intellectual system', which we might call the higher cognitive or higher psychological system. This was possible by "introducing the conflict into the definitive leading system, the active system of speech" (208). The importance of the role of speech in controlling behaviour was a subject that was to loom high in Luria's subsequent career, but he does not spell out his views here.

He opts for two main experimental approaches, the *conflict of setting* and the *conflict of defection*. The former, borrowed from Pavlov, is about choosing between two close options, known in philosophy as the dilemma of Buridan's ass. (Here Gantt renders the medieval philosopher as 'Buridanov'). It can result in a

conflict of purpose. The latter involves the stress, which the subject demonstrates when faced with diversions that deflect him or her from the task of solving a difficult problem, and the impossibility of solving the problem may even be part of the design.

The first trial of the former method involved moving the hand upward or downward according to signal – a red or yellow colour. Contrary to the expectations aroused by Pavlov's analogous experiments with dogs, humans reacted more calmly when presented with intermediate colours. "In place of impulsive pressure, the subject forms a link between his behaviour and speech, and begins to reason out the inhibitions of his reaction and finally gives an organized response – for example, making both movements, or perhaps none" (210). Humans have more complicated means of adjusting than other animals, and the experimenter has to reduce the options available to such forms of rationalization to achieve the desired conflicts. Speeding up the required tempo of response did bring some disorganization of behaviour but only in those subjects considered as having a hyperexcitable nervous system. Abrupt changes in the tempo demanded, however, produced perceptible conflict and disorganization, but hardly ever an instance of acute conflict.

Another experiment used bilingual subjects and verbal stimuli from both languages. The subject had to respond to the stimuli in the same language. Here the conflict was introduced via the sudden change of language. Sometimes the subject's response indicated that he or she did not grasp the word during the switch. Since the words were isolated from a meaningful context this could not, however, be attributed to an emotional response. Delays and inappropriate answers were symptoms of the disorganization of the associative process, and sometimes involved impulsive reactions or perseveration. "The conflict elicited very often produces considerable shock to the subject's higher speech processes accompanied by a rupture of the 'functional barrier'... In a series of cases we observed that, during acute conflict, inhibition of verbal reaction is connected with the fact that excitation is directly transferred to the motor sphere..." (220). Such disturbances were "evidence of weakened participation from the higher psychological system and of a return of the reactive process to a primitive diffused

state” (223). The experimental design not only took preemptive action against the rationalizing option of the speech system, but also challenged its very link with meaning, thereby denying both its conventional flexibility and escape routes.

The prescribed activity in experiments with *conflict of defection* should likewise tax the subject’s ability. Indeed, it should exceed the limits of his or her capabilities.

[In the ensuing] result of such a collision between the attempt at activity and the impossibility of achieving it, between hope and weakness, inevitably brings about a state characterized by confusion and affective disorganized behaviour... The conflict of defection should be [seen as] directly linked to the problem of neurosis. There is not a single neuropathologist who would not point to social or biological weaknesses of the subject as the basis of the entire series of psychoneuroses. Alfred Adler constructed round this conflict a whole system that greatly aided us in the comprehension of neurotic mechanisms, [i.e., the impossibility of compensating for the deficit] (224-5).

In Luria’s attempt to obtain such a result ‘synthetically’, however, the subject was led to believe that success was possible, otherwise he or she might reasonably decide not to take part. Luria found the experiments of Lewin and Tamara Dembo to be excellent models.

Luria’s experiments again involved verbal responses to words, and again these involved “limited associations”. The first experiment required giving an example of a relevant ‘part’ in response to being given a ‘whole’. Most wholes were easy to find such parts for, but there were a few ‘provocative’ examples, which seemed more like ‘parts’ themselves. Luria compared two groups, one of 15 students, the other of 30 neurotics. In this initial test the results of both groups coincided. The ‘provocative’ words elicited excitation in the motor sphere. The intentional sphere, involving the process of speech, was also implicated and produced excitation of the speech apparatus. “The process is characterized by the production of activity and a transfer of excitation to the speech sphere”. “The intention to answer, meeting with the impossibility of giving a ready response produces a

diffused, disturbed neurodynamic reaction.” (229). Even the passive left hand shows disturbance. Another experiment required the subjects to give lists of birds or fish. As the answers began to dry up, motor actions became disorderly and impulsive, rather as one might expect.

Attempts by other authors to arrive at general laws for these processes had failed, thought Luria, because they failed to take account of the structural dynamics of the situation, instead preferring to concentrate on what he called ‘subjective material’. He concluded, however, that “two types of conflicts [involving simple intellectual processes] can create the same symptoms that are usually the result of affects and affective traces. These two types of conflict were most closely connected with two types of mechanism” (236). These two types were represented by the conflicts of setting and defection.

In both cases the fundamental fact was the delayed activity. In the first of these cases, the delay in activity came about after having been formulated and prepared as a motor response. In the second case, the intention was simply reinforced by inhibition, as the intention was not formulated as a prepared reaction. [This delay or withholding from activity] is the chief mechanism of the affective processes, and it is precisely with this that those symptoms characteristic of the presence of the affective process are connected (*ibid.*). [The combined verbal and motor method] makes it possible to establish the presence of the affective process – and not only this – it also enables us to show what type of structural conflict it corresponds to (236-7).

Luria admitted that these conflicts “arose within the borders of a very *limited system*, and usually *did not extend into the entire personality*” (239). Although such ground had been covered in Luria’s ‘real-life’ experiments, he wanted to address the issue of obtaining an artificial, but more stable and more intense disorganization of behaviour. This he did in **Chapter VII**, ‘Experiments with Artificial Neuroses’ Again he refers to Lewin’s experiments. Luria admitted that in his own experiments he very rarely obtained an overreaction from the subject, despite “the delay or limitation of which should actually have produced an acute reaction on the part of the individual” (240). The best way of resolving this

difficulty was by using hypnosis – again with the assistance of Zabrezhnev and Varshava. The structure of the previous ‘artificial’ experiments was maintained, for example, by suggesting during hypnosis an obligatory verbal activity, which is then replaced on waking by a different one. These experiments were undertaken in 1926 and 1927 with about twenty subjects.

Again Luria used Leont’ev’s findings that “every emotional complex creates a certain tendency to reproduce itself in a chain of associations ... [and that] in different subjects the chain of associations dominates different objective structures” (243). In the first experiment described, a group of subjects received the hypnotic suggestion to think of the names of birds. Their performance in the following free chain association was compared with the free associations they had given before the process of hypnotic suggestion. Although some individuals were instructed when awake to include the names of, for example, fish or trees, but not birds, all the subjects peppered their free associations with the names of birds. They even rationalized this in the form of memories or confabulations. In one case the suggested theme was ‘square’. The response was, “The first thought that came into my mind was that in the square they feed the doves” (245). Although there is no apparent conflict, the subject feels compelled to override the conscious instructions. “This series convinces us of the stability of the suggested tension”. Often the tension is more evident, in that the hypnotically suggested verbal category “appears as a foreign body – and then the individual begins to struggle with it, tries not to discharge, but to inhibit it as something unconnected, foreign, onerous” (247). A subject refused to continue the associations after the following vivid example. “Raven – stork – goose – duck – store – poverty – word – time – Mary Pickford – red – beyond the sea – they flew – storks – I do not want to say anything more... (Why do I tremble! I am shaking so that even my hands tremble!)” (249). Here “we succeeded in creating a model of the compulsive state, which furthermore evoked independently those conflicts, which completely destroyed the normal course of neurodynamic processes, and from which a neurotic tendency usually develops” (251).

To preface the second set of experiments Luria writes:

In the clinical practice of medicine it has been frequently observed that the more acute attacks of fear are obtained when we try to prevent the patient with an anxiety neurosis from completing his compulsive activity... [Here he refers to Freud]. This fact leads us to believe that just here in the more acute forms of conflict, which arise from the inhibition of the compulsive tendency, we may approach the mechanism of the affect more closely. In the process of affective disruption some fairly powerful system of activity (usually connected with the subcortical apparatus) falls under the sway of the inhibition. The conflict arising is the more intense the more insistent the arrested tendency and the more categorical the inhibition. The tension produced in the neurodynamic system strives to escape along the path of inappropriate innervation, the appropriate exit being closed. Thus are created the symptoms of an intense diffuse excitation, characteristic of affect (253).

Experiments with inhibited compulsions were designed to create a model of this structure. Hypnotic suggestion of certain words, accompanied by the suggestion that they cannot be said, enabled Luria to examine these mechanisms that result in symptoms resembling motor aphasia. An example of this hypnotic suggestion is the following. "When you come into the experimental room and sit before the apparatus, you will want to repeat two words – red and blue, red and blue. However, you will not be able to say them, though they will continue to be present in your thoughts" (254). When asked to repeat the names of colours the responses were normal, except that those to 'red' and 'blue' were inhibited and resembled the most severe cases of affect. When the suggested conflict was hypnotically removed all the responses returned to normal.

Luria concluded that the experiment had provided examples similar to those of compulsive neurosis. Forms of substitution were evident including 'extrasignalling' or stereotypy. Some verbal substitution was attempted e.g. 'rose' but often completely unconnected words were used. The subjects did not attempt to break through the motor dam; instead they sought new connections, new speech exits, an appropriate 'intellectual' exit.

The conflict is displaced from the motor sphere to the connecting one, and the substitution begins to take on an entirely new character. From the senseless

substitution by alliteration, which are only trials in pronouncing the beginning of the word, the subject goes over to a rational replacement of the forbidden colour by others – instead of red: rose, carmine, violet – or [shifting to] a replaced image – ‘red handkerchief’. These replaced images help to determine the subsequent associative series, replacing the peripheral conflict by a centrally reconstructed series. Without doubt, here we dealt with two very different compulsive states, and it is very important that we [already] had two special [theoretical] structures for the neurodynamic processes.

The movement of the inhibition from the motor system to the connecting, coupling-up one relieves the individual personality of open conflict and avoids that affective rupture which is inevitable in the presence of inhibition of the already formulated compulsive activity (264).

This central displacement relieves the affective disturbances and leads to further questions that are considered in **Chapter VIII**, ‘The Structure of Conflicting Processes’. These conflicts - of setting and defection - showed a great functional difference. In the latter it even proved possible to transfer the conflict from the motor sphere to the connecting system where it becomes isolated from any direct disorganization of behaviour. “Many processes we have observed in the experimental conflicts bring us strikingly close to the phenomena of aphasia” (269-70). The first set of experiments in this chapter, ‘Experiments with Conflicts of Aphasia’ lead on directly from this observation.

Aphasia is an ideal disorder for the study of these problems. Not giving any constant neurodynamic disturbance, and often accompanied by few changes to the patient’s practical behaviour, aphasia enables us to observe the acute forms of conflict at the point when the patient is returning to his normal speech activity. The conflict during motor paraphasia and cases of amnesic aphasia often has an acutely expressed character of ‘conflict at the motor termination’. The patient understands the word he wants to say, sometimes pronounces the first letter of it but is not able to utter the word itself owing to inhibition during its final formation and pronunciation. This is why it is possible to observe in the general behaviour and [form of] excitability an acute neurodynamic disorganization that is clearly dependent on the actual speech conflict. It is possible to observe ... in aphasia the

exact fixation of the naturally occurring conflicts, here manifested in an unusually well-defined and isolated form (270).

It is also possible that we could experimentally move the conflict “from the motor sphere to the receptory-connecting area. We can arrange the experiment so that the patient encounters an obstacle, not during the reaction to a word, but during the reception and elaboration of the given stimulus and the search for an appropriate reaction. Those changes we obtained in the neurodynamic processes will be especially instructive because they occur in the subject’s ‘own’ motor conflict, and also deprive his behaviour of its ‘normal’ organization” (271). In these examples Luria expands the concept of the ‘connecting’ sphere to that of the ‘receptory-connecting sphere. He later notes that this sphere has “a wealth of functional possibilities” (289) [see following page for the full quote]. Although he was speculating about the precise neurophysiology involved, it does seem plausible.

In collaboration with M. S. Lebedinskii, Luria studied many aphasic patients (cf. Lebedinsky and Luria 1929). He found that, as in his previous experiments, “the receptory part of the process [in these aphasic patients] was completely lacking the neurodynamic disturbances that were so active during the conflict that arose in direct proximity to a motor terminal”. He thought this could be explained either by “some kind of ‘barrier’ impenetrable to the conflict arising in the prepared central process,” or that “the connecting system itself is constructed according to another principle” (280).

Luria next set out “to create a model of sensory and motor aphasic phenomena in one and the same subject, guaranteeing the presence of excitation in its active tendency in both cases”, i.e., at the very beginning of the receptory as well as at the end of the motor phase (ibid.). In this section, ‘Experiments with Displacement of Conflicts in Speech Difficulties’, Luria selected non-aphasic subjects to perform associative language experiments where the subjects were not very fluent in a given foreign language. Here “we could count on obtaining an acute conflict of defection, produced by a collision of the setting of the associative response and the insufficiency of vocabulary. We could certainly expect that the

structures of the conflicts produced were far from being equal” (280-1). In some cases the lack of vocabulary led to reactions to be found in amnesic aphasia, in others there was a wide variation of reactions as the subjects attempted to understand the word rather than respond to it. This had the characteristics of receptory conflict. This latter displacement occurred in the majority of cases involving the ‘cultured adult’.

Here “we begin to understand how the conflict displaced from the motor area plays the deciding role in the preservation of the personality... For this reason, the leading role among those mechanisms that bring about control of the actual affect should unquestionably be taken by the mechanism of the displacement of the conflict, which isolates it from the motor sphere” (288-9). Luria concludes that the likely explanation for this is that the receptory–connecting system and the effector system “play functionally unequal roles in the activity of the organism, and they control noncomparable structures... The first system in the cultured adult is isolated from the motor area in such a way that the excitation beginning in it is not directly transferred to the motor apparatus, but only when the elaborated process is completed” (289).

This division of all the activities into two strictly separated phases is characteristic of the behaviour of every adult. Thus in normal behaviour one feels that there is some ‘barrier’ between the two phases that obstructs the direct transfer of excitation to the motor area, thereby allowing the organism to prepare itself for activity, in order that it may then complete the prepared connection with the organized motor act. The fact that the connecting apparatus is so extremely labile and has such a wealth of functional possibilities, whereas the motor apparatus is comparatively simple, is perhaps the reason why every conflict occurring in the latter disrupts normal motor activity, while the same conflict, when displaced to the receptory-connecting terminal, is successfully utilized by complex psychological mechanisms and remains isolated from any detrimental behavioural influence (ibid.).

Here Luria broaches the issue of a hierarchy in the brain, and furthermore one that is associated with educational and physical maturity.

The final section of the chapter concerns ‘the neurodynamic type’ as it affects the relationships between the motor sphere and the receptory-connecting area. Luria sought to examine how the reactions of subjects suffering from various disorders and diseases differed in this respect from the reactions we have already seen. These subjects included those of a labile or excitable state considered pathological, those suffering from pseudobulbar paralysis, Parkinson’s disease, neurasthenia, neurosis, and hysteria. In most of these subjects the so-called ‘functional barrier’ between the two systems did not operate normally. Usually shifting a conflict to the receptory-connecting system isolates it from the motor system. In the labile motor system, however, “such a shift ceases to play its role – especially in the diffused structure of behaviour that we see in neurosis. Here the conflict freely passes over into the motor sphere, at the same time disorganizing behaviour. We have arrived at the fundamental neurodynamic mechanism lying at the basis of neurosis...” (300).

This passage which optimistically looks forward to Part III is consistent with having been delivered to America in 1929. The change of emphasis on the importance of the ‘functional barrier’ as distinct from that of the qualitative difference between the systems, although in itself no contradiction, shows that Luria was still in the process of developing his own ideas on the issues.

There is further speculation to be found in **Chapter IX**, ‘The Dynamic Analysis of the Conflicting Process’. Here it seems that the issues concerning qualitatively different systems and the functional barrier are quite rightly subsumed within larger issues.

We are undoubtedly justified in speaking of the presence of certain levels of organization in the behaviour of every personality. While the more primitive of them are already regulated at the earlier stages of their development, the more complex and difficult ones are also more labile even in the very complex human organism. The [possibility of] estimating the lability of the neurodynamics of the personality may arise not only in relation to the ability to keep the conflict going on within it outside the motor sphere ... but also in relation to [the particular] stratum

of behaviour where the conflict ... begins to produce its disintegration and disorganization (301-2).

[NB. 'Labileness' is a term for the tendency to slip and also to change. Hence the word for the tendency to instability may sometimes be ambivalently, but legitimately, associated with the ability to make many and complex changes].

[The organization of the behaviour of the very young is not complete.] It appears clearly organized if we observe it at a primitive level, for example, in the act of sucking, and disorganized if we then look at the more complicated levels concerned with the activity of the cortical apparatus. In the adult the disorganization of behaviour is shifted to a much more complicated stratum of behaviour and is under the influence of entirely different factors and therefore characterized by a qualitatively different structure. In our previous analysis of the affective processes ... the whole series ... of the simplest movements were completely attainable by the subject, ... while the inclusion of the more complicated, mainly higher associative processes inevitably produced a rupture and severe disorganization of behaviour (302).

How do we find that level of complexity in the adult that makes such changes possible? It is not an even or gradual process of change.

[On the contrary] the process of development actually has a complicated and ... dialectical character [so that the role of] key factors changes at different levels [of development]... [In the adult] all the elementary behavioural functions in which speech plays a part are stable and organized, and excluding its participation makes the behaviour much more unstable. The satisfactory explanation of this is the fact that the inclusion of speech alters fundamentally the organizational principles of behaviour. It changes the natural forms of the gradual organization 'from below' to the cultured forms of behaviour 'from above'. That which was previously the more complicated and difficult level of behaviour now not only appears as the more stable level, but rather the system actually playing the organizing role in relation to other levels of behaviour... Our analysis must necessarily take into account this dialectical approach, calculating the new context of the given phenomena and the new organizational forms and principles at each stage of development, starting

from the analysis of the new leading factors, and the ensuing new structure of the given phenomena.

This makes us recognize that we are only at the very beginning of that path which can lead us to a satisfactory study of the degree of neurodynamic lability of the personality. This question presupposes a prolonged investigation of those actual [interrelations and] correlations existing between the different functional systems and that structure characterising behaviour at the various stages of development (303).

In this passage we see in a nutshell the argument presented in Vygotsky's 1934 article, *Psychology and the Theory of the Localization of Psychological Functions* (cf. VII, iii, 224-6), first translated by Luria with his own accompanying article (Vygotsky 1965, Luria 1965). This approach is, I believe, as important as any other theory to be found in either Luria or Vygotsky. The enormous changes of which Luria speaks are most evident in the field of child development, and it is likely that Vygotsky also played a key role in the development of this approach. Luria's involvement in this field of work was not only vital for Vygotsky, but also for Luria in widening the scope of his thinking. On the other hand, the development of Luria's own work seems to have led him independently or perhaps together with Vygotsky to this theory - for Vygotsky too studied in what are conventionally seen as 'Luria's' areas. I have amended Gantt's translation in the passage above not only to make it more comprehensible, but to make this theory more explicit and recognizable for what it is, rather than what it has been hitherto – at least in English! We will return to look at this theory especially in the section dealing with the development of the functional systems approach. Here it is important to note that Luria's comment, that "we are at the very beginning of that path which can lead us to a satisfactory study of the degree of neurodynamic lability of the personality", seems in the circumstances to be an understatement of the first water. Here he has come up with a theory of developmental neuropsychology that has surpassed every previous one, including those of the leading figures of the Gestalt school, Kurt Goldstein and Kurt Lewin. But, of course, Luria's whole series of investigations was leading in this direction; the study of 'lability' was intended to unlock many theoretical doors and also to

find appropriate treatment for many of those sufferers of its various pathological forms.

For what Gantt translates as “the dynamical stratified analysis of the conflicting processes”, i.e., a dynamic analysis of conflicting processes at [separate] levels, Luria used material from the investigations of the previous chapter. There is no evidence from Lebedinskii and Luria’s German article of 1929 (received by the journal on 9 February 1929), that Luria had yet conceived of his new theory. Nor does it appear in the various presentations of his method in 1929. One therefore assumes that these preliminary ‘pilot’ studies are not a re-analysis of existing material, but a new series of investigations probably undertaken after his return from America in 1929. Luria’s reference to a 1930 publication on page 325 gives us a general time frame within which the theory was developed and the experiments undertaken.

“The simplest example shows us in which comparatively primitive strata ... the behaviour of a neurotic acquires a conflicting character” (Luria 1932a, 304). In this case the neurotic was a ten-year-old boy. His task was to tap rhythmically on a pneumatic plate until he received a signal to stop. The signal produced, instead of the expected inhibition, “an acute impulsive rupture of the motor activity” (ibid.). In other cases neurotic subjects were not able to perform the simple rhythmic tapping without destroying the rhythm. “Here these reactions remain regular in form, but disorganization is reflected chiefly in the disturbed rhythm and intensity of the [response] curves [on the recording graph], i.e., in the loss of that standard character that distinguishes this process in the normal [subject]” (307).

Yet responding to a signal is not the most elementary act, despite the views of a generation of psychologists. Luria notes that this will be examined in more detail in Chapter X, and explains briefly why this is so. “The reactive response to the internal stimulus presupposes primarily not an internal coordination of the separate links of a spontaneous system of movements, but movements [whose control is] transferred to an external agent. Even this is a much more complex

action than those that are not connected with spontaneous external movements” (310).

Luria ran a series of tests on a number of neurotic patients, as a result of which he was able to place them in a series of groups that were characteristic of different grades of lability of the nervous system. They took part in a test where they were asked to respond slowly to stimuli. The ability to inhibit movements that this required often proved short-lived and attempts to do so sometimes led to the complete destruction of behaviour. “Under these circumstances the inclusion of speech is not a strong enough means for controlling the neurodynamic process” in these neurotics (315). Luria summed up this section with the recognition that, “Conflicts connected with even the most primitive levels of behaviour may cause acute disturbances” (319).

Despite all these difficulties, Luria was convinced that it was important to create a differential analysis of the mechanisms involved in order to form a genuine and useful scientific typology.

In studying the degree of organization of behaviour at different levels, and in substantiating the effect of conflict on the failure of the reactive process in ... [relation to these levels], the possibility came nearer of expressing individual differences in several dynamic units... Establishing that the behavioural disturbances in the different subjects are clearly expressed only at a definite level of activity, we are able to describe the lability of this behaviour in special [measurable] stages, ... thus making our individual analysis dynamic... We are [also] in a position to substantiate the analysis of [such] disturbances, and to speak of the functional peculiarities of that degree of lability with which we are dealing (324).

Precisely how far Luria had got in this process is not made clear or concrete at this stage. One speculates that the measurable stages were to some extent and inevitably provisional. But Luria was in no doubt that they were required.

In our comparative typological analysis we must each time decide two questions. In what degree is the neurodynamic lability manifest in a given subject and,

secondly, what structure characterizes the usual failure of the behaviour? The answer to the first question provides a dynamic analysis of our subject's reactions at definite levels of his behaviour. The second problem is decided by a study of the following: the qualitative nature of the disturbance, the stage it is at, what its reciprocal parts are in the disorganization of the somatic and vegetative systems, and whether the inhibited and deformed state of the subject is passive or active, intentional or [in the process of] formation (ibid.).

Although such a formulation appears as an agenda, which is indeed what it is, rather than exhibiting its dialectical structure, the dialectics are implicit. Luria does add that these are only some of the aspects that need studying. "The whole dynamic analysis of the typological differences is the task for the future, and here we shall express only a few generalities relating to the problem" (325).

A comparison of two cases of hysteria illustrates the value of Luria's approach

The differences between these two cases consist in the degree of neurodynamic lability, but that is not all. The varying activity of the conflict, the varying extent of its irradiation, and finally the varying participation of the motor system in the conflict create a structure specific to each case, and force us to think about the different structures of hysterical behaviours.

We are convinced that under the general term 'hysteria' very divergent neurodynamic processes are included. The detailed neurodynamic investigation helps us to unveil the exact picture of those disturbances that are usually brought together under the label of hysteria, and to describe exactly those differences, and finally to classify them as groups (327).

Nevertheless Luria admitted that the processes under study were not entirely comprehensible.

We are convinced that the laws of this disorganization will be fully comprehensible when we are able to give their genetic analysis, approaching their study through the [developmental] method. Only by a careful investigation of those successive stages through which the organized behaviour of the child passes in the process of transition to adulthood, can we thoroughly understand to what degree the facts

observed by us in affective and conflicting processes originate at some more primitive stages in the development of neurodynamic processes” (ibid.).

On the other hand, “the human does not only experience failure in [the organization] of his behaviour; he also tries to master it, to control it... The individual descriptions of the laws we have given are comprehensible only when we consider in detail those means that the human applies in controlling his behaviour, and those stages through which his mastery of behaviour passes”. This is also central for therapeutic work. “We believe that only experiments dedicated to the genetic analysis of organized forms of behaviour and the experimental model of their control can give us a foundation upon which we may make a contribution to the contemporary growth of knowledge” (328).

Thus the inner logic of Luria’s own investigations led him to the study of forms of child development and their relevance for studying the mind, its problems and its possibilities.

II

Part III

‘The Genesis of the Reactive Processes and the Psychophysiology of the Control of Behaviour’, i.e., Part III, contains three chapters. **Chapter X** is titled ‘Development of the Reactive Processes’. Having recognized the key role of development in understanding neurological structures and their breakdown, Luria examined current theories. He concluded that throughout the world writers tended to opt for the view that affect does not create new forms of behaviour; on the contrary it throws the individual back on old mechanisms. Often this was to the extent of seeing the individual as reverting “backward through many generations, so that affect returns to ancient phases of behaviour and neurosis regresses toward an archaic stage of development” (331). More plausibly, in neurological terms, it represented “a transfer from the cortical type of excitation to that which is connected with the subcortical ganglia. These form the ancient parts of the brain and are the seat of archaic types of process” (331-2).

Luria considered this approach to be both “daring” and containing “the golden kernels of dialectics”. He does not refer to Freud by name, but the language is so similar to that used in the preface written by Vygotsky and himself to Freud’s *Beyond the Pleasure Principle* (cf. IV, I) that it is undoubtedly Freud that he is considering. (In my discussion of this preface I tried to offload the rather extravagant ideas and language onto Vygotsky, and since they also sound so unlike Luria in this context, I see no reason to revise my views). At this point, however, Luria urged caution. True, disorganization did not unravel along individual *ad hoc* paths. But “we may suppose that the process we have studied will occur not so much according to a scheme of simple regression, but rather to a scheme of reversion in which the archaic and new forms [of behaviour] in a sense change places because, even in the newly destroyed form of behaviour, the higher mechanisms continue to play a role, even though a ‘perverted’ one. This is why in each destruction we must necessarily expect a return to some of the former stages of development” (332). Here Luria’s position is consistent with that of Hughlings Jackson, though, as previously stated, he probably did not yet have access to the relevant writings of the latter, which explains the absence of some of Jackson’s finer points. Luria here also adds the qualification that “these manifestations of the reappearing archaic forms will naturally be different in people of different cultural states, different types and different individual peculiarities” (ibid.). Here we see the influence of cultural-historical theory, which we will discuss later in a more appropriate context.

“Consequently, in our investigations we must not look for a simple regression of behaviour ... representing a primitive form of development. Yet we still face the important issue of studying these former stages of development in order to understand the characteristic mechanisms forming the skeleton of those higher regulations of human behaviour which disintegrated” (332-3). “We must first take up the ontogenesis of the reactive processes and turn our attention to the study of the neurodynamic characteristics of childhood” (333). Luria raised the key relevant questions: do the reactions of child and adult differ in structure?, if the ‘functional barrier’ is lower in the young child “under what conditions is it destroyed and to what mechanisms does it belong?” (ibid.).

The main proposition, which seems to us proven by a series of experiments, is that human reactive processes complete their development not by the gradual combination of earlier given mechanisms, but, on the contrary, ... by the conflicting characteristics being transferred into qualitatively new phases subsumed by the new regulating mechanisms. Thus the reactions of the young child differ fundamentally in their structure from those of the adult... Every behavioural act in the young child has a direct character and the excitation arising manifests a tendency not to be restrained, but to proceed to its motor termination. Secondly every reaction exhibits the ability to bring with it an inappropriately large mass of excitation into the active process... [These characteristics] reveal to us the genetic roots of those neurodynamic deformities found in the disorganization of human behaviour in the states of affect, conflict and neurosis (334).

The experiments in this chapter attempted to discover the development of organized forms of behaviour at different neurodynamic stages, and their different levels of complexity.

The first experiment required pre-school children to press a pneumatic apparatus at a speed of their choice. This procedure actually “presupposes a fairly high development of the cortical processes; only with a fairly well-organized action of the motor cortex and development of higher cortical automatic mechanisms could we reckon on obtaining an accurate [response]” (335). For this reason children as young as thirty months were prepared for the experiment by watching older children perform it. Nevertheless in the young child the process associated with the cortex transferred rapidly to subcortical mechanisms which involved diffused processes. “A series of equal, regular pressures, readily and automatically given by the adult, evidently requires a very high organization of the cortex that is lacking in the child...” (336). The children’s reactions are typical and are seen in children up to the age of seven. “In adults we may easily evoke them by instructing the subject to produce the reactions at maximal speed” (337-8).

The following experiment involves reactions to a signal. Again, as Luria has already explained, this is a far from elementary process. Here he adds an interesting comment. “Authors who have considered psychology as a science of

reactions have evidently taken for granted that this process is exceedingly elementary and totally unchangeable during the course of development as a whole". He also made the point that "the development of the reactive processes from child to adult does not occur in any sense by the quantitative improvement of the process, but through a qualitative change in structure overcoming primordial diffuseness which is transformed into a new, intricate, functioning, organized structure that controls the reaction" (338). No wonder leading followers of the Pavlovian orthodoxy instituted by Stalin's regime at the end of the 1930s, men like A.G. Ivanov-Smolenskii (whose work is mentioned, not unfavourably, by Luria in this book), seemed to enjoy humiliating Luria in the early 1950s - for almost incidental comments, such as the above, robbed Pavlovian theory of any claims to pre-eminence whatsoever.

The experiment seems initially to have been undertaken by Lebedinskii and replicated by Luria with very similar results. Lebedinskii even came up with a formula for the 'coefficient of inhibition'. Precisely whose material was used is left unclear by the English text. The subjects were aged from two and a half to seven or eight and they were to respond by pressure to a sequence of signals. Sometimes even beyond the age of five the reactions "showed that *each signal mobilized a large amount of excitation*" (338). The spontaneous pressures of the youngest "are hardly at all connected with the external signal, but ... may even be inhibited by it... [At this age] it is still very difficult to bring about a differentiation of an organized reaction from the overall excitation". By three and a half the child typically produces separate pressures coordinated with the signal, but "is not able to inhibit the succeeding excitation" (339). "All the further development of the reactive processes consists primarily in the development of [the] ability to restrain the remaining impulses and overcome the amount of excitation caused by the stimulus" (339-341). This requires additional mechanisms. Another experiment showed that children of six or seven were incapable of organizing any form of delayed response. This is crucial. For "all higher psychological behaviour is possible only ... when the solution of problems can be deferred for a time. The problem is temporarily isolated from the motor sphere and from the seat of those preliminary internal trials which are not

immediately reflected in the motor system. Here lies a [key] mechanism of the intellect” (346-7).

Perhaps other forms of experiment could help explain the behavioral transition from child to adult. Luria experimented with choice reactions. Here he also included material collected by P. S. Lubimov. In one experiment Luria even attached a small light to the index fingers of children and adults so that its pointing responses could be ‘cyclographically’ recorded. Again the cyclograms showed two phases in adult reactions, namely preparation and then motor fulfilment; children’s remained impulsive (349ff).

Luria looks at evidence from Lewin’s photographs of infants clinging with the whole body, and the experiments of G. E. Coghill, reported at the 1929 International Congress of Psychology on diffused movements of the human embryo. These confirmed Luria’s views about the diffuse reactions of young humans. Even voluntary activities show a similar structure. In experiments performed with another lifelong colleague, A.V. Zaporozhets, (1905-81), a one-time actor with Eisenstein, Luria proposed that children make different kinds of drawings, with either ‘neutral’ or ‘emotional’ content, for example, ‘a kind aunt’ or ‘war’. The motor actions registered by a six-year-old exhibited intense motor discharges for the latter, but in older children the motor activity barely differed between the two drawings. Luria links the difference with two facts. “On the one hand, that direct character of the reactive processes, the tendency of all excitation to be immediately associated with activity... On the other hand, the higher psychological mechanisms, particularly that of speech, which have not yet begun to play the regulating role in the child that it later takes in adolescents and adults. In the young child speech is not a perfect means of making judgments and planning, and the absence of this preliminary phase gives a primitive form to the intellectual processes of the child...” (359). Direct impulsiveness is restrained, yet remains an element for many years, and may return unexpectedly in adults.

How does one arrive at a neurodynamic age? Chronological age, physical maturity, and even ‘intellectual’ age are not necessarily guides to this. In the final analysis it comes down to the issue of the control of behaviour. It therefore

“ceases to be a question of biology and becomes one of cultural elaboration on a biological basis, i.e., a problem of psychophysiology” (362). Some valuable work had been done on this in the field of nervous excitation by, among others, A. G. Ivanov-Smolenskii (!). But a second tier of methods should also be used in conjunction with this, namely the area concerned with the control of ‘voluntary’ activity, which is what Luria was currently occupied with. These experiments would require “a series of problems of increasing complexity and the inclusion of more intricate mechanisms” (ibid.). The role of the mechanisms involved in the functional barrier would also require explanation. Clearly this would be, and was to be, a long-term project for Luria. The role of speech, which Luria has said above, almost in passing, was very important, was indeed to prove crucial to this work. Allied to this question was the need to characterize neurodynamic types. Any symptomatology of the latter would have to be based on a dynamic analysis, rather than on existing ‘static’ forms of classification. This Luria correctly terms a “dialectical analysis” in the provision of a typology (364), one that included a genetic study of the phenomena.

Although much, if not most, of the work that would validate Luria’s approach would lie in the future, it is evident that this work needed to be envisaged within an overall theory. Aspects of this approach would need to be amended or replaced, but in general it gave the opportunity to proceed on many fronts and in theory simultaneously. Luria chose next to look at one of his key concepts, and **Chapter XI** is titled ‘The Nature of the Functional Barrier’. He asks:

Is it inborn, congenital, only gradually appearing in the process of unnatural growth, or is it a product of education, becoming manifest in combination with certain new cultural contributions in human neuropsychology? Must we consider it a morphologically formed apparatus? Or is the ‘functional barrier’ a functional conception that conceals no new morphological elaborations, and concerned only with the combination of different systems, or other structural processes of behavioural neurodynamics including new and culturally higher psychological relations that influence the course of the reactive processes? (367).

To which one adds another question. These are such large issues: is it possible for Luria to solve them?

The supposition that the development of psychological functions is necessarily connected with the growth of new cortical formations is a barely tenable conjecture. The facts are rather that we know many more complex forms of development connected to new combinations in the use of those same morphological elaborations that are accompanied by a change in their functional significance. Those modifications that are encountered in the development of child behaviour may be related to processes of the second type. The inclusion of social surroundings, speech acquisition, the use of instruments, and the transition to culturally new forms in the organization of individual behaviour – does this not change the structure of psychophysiological processes just as much as the appearance of some purely morphological alteration of the nervous apparatus? Moreover, in this complicated organization of behaviour, these functional changes are often predominant, and we know scores of cases whereby it was possible to compensate for serious defects in the coarse morphological structure of the nervous apparatus (369).

As a materialist Luria does not deny the role of the morphological structures. But “the higher forms of behaviour as well as the primitive can be functions of an identical morphology in the brain; cultured behaviour does not require a new brain morphology, and the brain of the savage may be morphologically identical with that of a member of the Academy of Sciences, [- a point made by Herder in 1784!]. The most intricate psychological elaborations may be contained in a plan of functional structures, and the same functions used in entirely new combinations involving new and adapted environmental mechanisms” (369-70). Here Luria has laid out much of his theory, which we might consider as a contribution to the nature-nurture debate, but would then have been considered as implying an acceptance of the cultural-historical approach.

At this point Luria does finally introduce Hughlings Jackson, who – he states - concluded in 1884:

That the higher level of the nervous apparatus was inhibitory, restraining the primitive reactions of the older [subcortical] cerebral systems. This included the restraining and organizing role of the morphologically higher layers of the apparatus as well as the analogous role of the higher functional systems, creating the complex processes of biological and historical evolution. Jackson and Head in their work on aphasia pointed out the primary organizing role played by speech in the voluntary and emotional disturbances occurring when these complex functional levels were damaged. This exposition is of vital importance for us. Indeed our further discussion is based on it. Many experiments show the markedly inhibitory role of the cortex (370).

Here, it is almost as if Luria takes on the views of these masters just as his own research had led him to the same point and the same conclusions, and also at the same time when their work finally became available to him.

Luria refers to other recent Western research, including reports from R. S. Woodworth, Sherrington, Wilder Penfield and Philip Bard, which support the above comments on the effect of the weakening of higher cortical systems and could be consistent with the concept of the functional barrier. "We may assuredly expect that the action of the functional barrier will be closely connected not only with the participation of the new morphological layers of the cortex, but also with the inclusion of those higher functional systems indicated by Jackson and Head, which could be elaborated only in the most intricate processes of psychological development, and which play not only an inhibitory, but also a formative, organizing role" (371). This second reference to the concept of functional systems in respect to the work of Jackson and Head is striking.

Although in much later works Luria does refer to a collection of Jackson's texts published by Head in 1915 in volume 38 of the journal *Brain*, he does not refer to any specific publications by Jackson here (Jackson's major relevant works were republished in 1932). Therefore we must assume that Luria's knowledge of Jackson derives largely from *Aphasia and Kindred Disorders of Speech*, which frequently cites the 1915 collection (Head 1926). That this latter work became available to Luria about this time is confirmed by Michael Cole (1979, 210). It is

also referred to by name in this work (Luria 1932a, 7). Luria's reference on page 370 to the effects of chloroform on the cortex, immediately after referring to Head, echoes Head's remarks on the subject (1926, I, 487). But it has to be said that neither Jackson nor Head refers to functional systems. What then did Luria find in their work that played the same role as in his own? Indeed, what role did it play in Luria's own development of the concept? And, perhaps most important, in what overall approach was it embedded?

Neither Head nor Jackson is mentioned in any of Luria's three German papers from this period. Even in the article that has a special section on aphasia, the very article that appears to be the first to use the term *Funktionssystem* (Lebedinsky and Luria 1929, 474), there is no mention of them. I therefore conclude that Luria came up with this concept independently and in relation to his combined verbal and motor response method, in which the speech and motor parts acted together as one system. But this was a system that could disintegrate under pressure. It was something that developed in the course of ontogeny and was one of the higher cortical processes. I think that reading Head's work gave Luria the confidence to expand both the meaning of his concept and the scope of his approach. It seems quite possible that Luria obtained the book en route to America or actually when he arrived there in 1929. In *The Nature of Human Conflicts* it seems to be a relatively late arrival – virtually everyone else cited had already featured in Luria's writings, apart from some more recent supporting material from the 1929 lecturers, Lashley and Coghill. Head's work was not merely supportive, but radically accelerated Luria's moves towards a recasting of his overall approach.

First we will look at the material consistent with Luria's concept of functional systems, and then look at the context in which Jackson and Head embedded it. Head's book includes a section on Jackson's work and often refers to his ideas elsewhere. Head refers to an 1864 lecture by Jackson, which discusses two types of speech, intellectual or propositional and emotional or interjectional. Of these, Head cites Jackson, "Healthy language could be divided into two distinct forms, which may be separated by disease", and then adds himself "speech, apart from its articulatory aspect, is double, consisting of intellectual and emotional language, and it is the former that is usually disturbed in consequence of cerebral disease"

(1926, I, 35). More recently these two forms have been linked to the dominant and non-dominant hemispheres, and such separations are not disputed. To me they represent two different and complementary parts of a functional system, or indeed a *Funktionssystem* or system of [different] functions - as envisioned by Luria. 'Intellectual' or 'propositional' language played a similar role in Luria's work (cf. Jackson cited in *ibid.* I, 51). More importantly the many symptoms of the various forms of aphasia listed by Jackson (in *ibid.* I, 37, 40-3, 45) are almost sufficient in themselves to prompt the use of a concept like 'system of functions' to help explain the dynamics of such systems and their disorders. Head draws the following conclusions. "Speech, reading and writing are acquired at a period when the central nervous system is structurally complete... [Such] acts of symbolic formation and expression ... employed highly integrated arrangements, developed originally for simpler purposes. These in turn depend on the integrity of a series of arcs or circuits subserving processes on the most diverse physiological levels, the highest of which are to be found in the cortex" (*ibid.* I, 475). Here again we find functional systems, not of the 'complementary' type above, but ones that integrate many different activities across levels, and most consistent with Luria's 'prototype' of the combined verbal and motor functional system. [NB. The Russians too would probably have enjoyed the supplementation of the old reflex 'arcs' with 'circuits']. Jackson was a firm opponent of 'faculty' theory, especially as regards speech. Head amplifies this in respect of the localization debate when he argues against centres as such. Instead such apparent convergences of activity should be considered as "foci of integration" (*ibid.* I, 498).

There is as yet no evidence that Luria read Jackson directly in the period covered by this thesis. Certainly Vygotsky's later works give no sign of this, though possibly Luria was more fortunate, but he could only have read him in 1932 or 1933 at the very earliest. Jackson wrote of different kinds of centres at different levels, but not those considered in localizationist approaches tied to faculty theory. In his classic series of lectures from 1884, *Evolution and Dissolution of the Nervous System*, Jackson wrote, "(1) Evolution is a passage from the lowest and the best organized, that is to say, from the lowest, well-organized centres; putting this otherwise, the progress is from centres comparatively well-organized at birth up to those highest centres, which are continually organizing through life. (2)

Evolution is a passage from the most simple to the most complex; again from the lowest to the highest centres... (3) Evolution is a passage from the most automatic to the most voluntary” (Jackson 1932, II, 46). He also said, “A consideration of the effects of epileptic paroxysms lead, I submit, to the conclusion that the highest centres represent, through the mediation of the middle and lowest centres – that is re-represent – all parts of the organism in most intricate combinations” (ibid. 64). The concept of re-representation was to be important in Luria’s subsequent theoretical development. It explained why frontal cortical lesions led to systemic failure. It also explained why Luria was correct to see that the new systems did not operate on the basis of reflexes and instincts, but incorporated them in higher, more voluntary systems. That is, there was a qualitative change in adult human behaviour. Jackson adopted the concept of ‘levels of integration’ from Herbert Spencer – much to Spencer’s surprise. Spencer, of course, had borrowed this concept from Hegel. As Joseph Needham pointed out, “Dialectical materialism ... based itself on that very evolutionary progression which Spencer described with so much care. His successive levels of integration are allowed for in the dialectics of nature, as in hardly any other philosophy” (Needham 1986, 255-6). We have already seen instances of all these key points in Luria’s comments at the beginning of Chapter IX (Luria 1932a, 301-3). They were all to play a very significant role in his future work.

P. K. Anokhin, the physiologist and friend of Luria, followed at a short distance. Although he later used Spencer and Jackson’s concept of ‘levels of integration’ he did not do so at the time. His concept of the functional system was unlike Luria’s initial use of the term. Instead he saw it as a relatively self-contained physiological system in which feedback was central to its functioning (Anokhin 1935b, 52-5). [NB. I intend to publish an article that will explain his theory in more detail]. He wrote:

According to our concepts developed in 1932-5, the functional system is a selective integrative formation of the organism. It is a true unit of integration, which arises during the dynamic development of any qualitative activity of the organism as a whole. We have always emphasized [since 1935 –cf. Anokhin 1935a, 1935b] that the functional system is always a selective central-periphery formation and not

merely a formation of the central nervous system per se. The mere fact that this concept developed in our laboratory was the result of the difficulties that we experienced in 1932 in explaining the compensatory mechanisms of the organism as a whole solely on the basis of the generally accepted reflex arc, emphasizes its special physiological architecture. It turned out that systematic adjustments of motor acts in connection with compensation of disturbed functions proceed on the basis of continuous information from the results obtained. Consequently, to an extent the entire process of compensation acquires a *circular character* (P. K. Anokhin 1974, 190; cf. Anokhin and Ivanov 1935).

Amazingly Jackson foresaw this entirely in a series of published notes from 1868-9. He even spells out that “The unit of the sensori-motor management, by the excitation of which the movement results, must then have *organic* connections with the units of the sensori-motor arrangements, by excitation of which the movements before and after result” (Jackson 1932, II, 236). It now begins to look as though the concepts of feedback and self-regulation have roots not only earlier than Norbert Wiener, but also Anokhin and Bernshtein. Jackson adds (ibid.) “Similarly there must be an organic connection betwixt the two sensori-motor arrangements for the word ‘ball’ and the image ‘ball’ as the two are in indissoluble association”. This further example of an ‘organic connection’ has implications beyond that of the physiology of functional systems. It extends to language and human relationships, as Luria and Vygotsky were very well aware (cf. III, i, 72-3, & iii, 87). It is significant that the last publication of Macdonald Critchley, a postwar international collaborator of Luria’s, was a major biography of Jackson (Critchley and Critchley 1998).

Head made the important point: “That form of behaviour which we call the use of language has a history, and many of the phenomena of disordered speech resemble [though not strictly] the stages by which the complete act was developed in each individual” (Head 1926, I, 510). Again Head re-emphasizes the importance of the process of the development of higher cortical functions. More significant for Luria perhaps were Head’s comments on the role of language in differentiating humans from other animals, in regulating human behaviour. “When man learned to speak and understand spoken words, he acquired the power of

registering relations. Action was no longer determined by perception or unformulated mental responses... The use of symbols materially shortened the processes and extended his powers of thinking” (ibid. I, 523). “Logical thinking holds in check and diminishes affective and intuitive responses. An animal, or even a man under certain conditions, tends to react directly to the perceptual or emotional aspects of a situation; but symbolic formulation enables us to subject it to analysis and regulate our behaviour accordingly. We thereby gain the power of breaking up a situation for the purpose of selective action...” (ibid. I, 525). Of course, most of these points had been said, in one way or another, by Herder and his successors. Head’s reputation for analysing the effects of war wounds was as high as Goldstein’s; his field of aphasia concentrated more on language and disorganization, and his overall approach answered more of Luria’s questions and was also consistent with his own thinking. Therefore his words would almost certainly have carried weight with Luria. Above I noted that Luria’s comments on functional structures involving environmental elements would today be seen in terms of the nature-nurture debate, and would at that time have been seen in terms of cultural-historical theory. But here apparently Head is talking of those same ‘environmental’ elements, which he termed “symbols” in a *neurological* framework. He also talks of development within this same framework. Neurological thinking was clearly not isolated from that of other sciences and ideas. But the point is that this time round, when Vygotsky re-introduced the ideas of Herder, as mediated and amended by Potebnia, they took flight because they were not accompanied by various assumptions or speculations as to how the mind or the brain worked, but based on plausible scientific hypotheses that were rapidly becoming theories. Vygotsky, no amateur himself in these matters, was undoubtedly aware of the significance of this sea change, and he himself took part in investigations at the Clinic for Nervous Diseases and elsewhere. Whether their private correspondence will show if they saw things in this light remains to be seen, but I believe Luria’s neuropsychological investigations not only formed the basis of his own development, but formed a substantial and *necessary* complement to Vygotsky’s work. We will discuss this further in section III and assess the influence of Vygotsky’s approach on Luria. Chapter 8 will also show how the cultural-historical approach of Vygotsky was intimately related to the development of these theories.

Chapter XI continues with a brief look at the structure of the reactive processes in functional neuroses, based on material presented in Chapters VIII and IX. Luria found that, whereas increasing the intensity of stimuli to those with a functional neurosis led to a proportionate overreaction, normal adults, able to isolate their responses from the motor system, suffered no such problem (Luria 1932a, 373). He concluded that the structure of the reactions of hysterics was relatively primitive, and reported experiments done with Vygotsky and a researcher named Eidomov as confirming this view (377). During periods of excitability the functional barrier was destroyed. In those suffering from brain damage at birth and lacking higher cortical mechanisms, the higher regulation often proved to be absent. In such cases (largely based on material from Lebedinskii) reactions were different from those of disturbed adults, varying both in form and intensity. “The behaviour of the [oligophrenic] is not disorganized, because it never was organized...” (380). This insufficiency of the cortical apparatus accounts for the absence of a functional barrier. This “is connected with the activity of the higher functional systems” (384).

Luria found it possible to lower this barrier in normal adults by experimenting when they were tired. Also, instead of allowing them to focus on the experiment, they were talked to or given a book to read. “Such a functional exclusion of the higher cortical mechanisms [from the intended task] evokes a return to the primitive, diffuse type of reactive process and a sharp lowering of the ‘functional barrier’” (385). Nevertheless “the reactive processes of the [adult] human do not [present] the simple phenomena which might be governed by the laws of the lower reflex mechanisms” (387). They differ qualitatively, “in that they are constructed not only from *below* out of the simplest neurodynamic mechanisms, but also from *above*, according to the laws that govern the activity of the higher psychological systems” (ibid.). On the following page Luria makes further criticisms of reflex theory, leading the translator to refer to publications of the Pavlov school presenting the opposite view (cf. V, iv, 140). Luria continues, noting the “*instrumental*” nature of speech. “Precisely in the activity connected with speech we succeeded in observing the transfer from the primitive, diffuse and direct process to the process that split into two functionally different phases – the

phases of preparation and execution. By virtue of speech, the primitive impulsiveness is overcome, and direct attempts of adaptation are substituted by the preliminary connection in words; after this comes the motor execution". "We intentionally started this book with a discussion of cases of aphasia... In aphasia there occurs not only a simple dropping out of speech as a communicating function, but also the associated disintegration of the whole of organized behaviour..." (Luria 1932a, 389). This is markedly so in the elaboration of intention. Here Luria refers to two experiments from April and May 1930 with two aphasic people (390-1). Although in some cases actions may be isolated from the normal effector processes by the nature of the disorder, in other cases the functional barrier can be shown to be weakened. "This is shown by the prominent role of perseveration in aphasia; the whole structure of the paraphasic disturbance of speech with the jumbled and broken words can be explained from this point of view... We conclude that speech is a preeminent factor in behaviour", which is why the investigation of aphasia is so central to our work (393).

Luria notes the emergence of the functional barrier alongside the development of speech and the higher cortical mechanisms. "This leads us to believe that in the functional barrier we have not a natural mechanism, but one of cultural origin," but nevertheless having a regulating character (394). "We think that the inclusion of the systems of internal speech or the analogous systems of auxiliary stimuli in the reactive process is fully sufficient to explain the mechanism of the functional barrier". "By considering a reaction not as a mechanical habit, but as a mnemonic-technical act, realized by the inclusion of complex psychological mechanisms, we are able to illuminate the nature of psychological reactions" (395).

As we have seen, "a slight intellectual disturbance" may evoke a major disorganization in behaviour. The inclusion of speech as a regulating factor in the higher psychological processes explains how this can occur.

The reactive process gains a tremendous advantage by the inclusion within it of the regulating functional system, thereby becoming more plastic and independent of the mechanical conditions, but at the same time this lays it open to accidents that

may occur in the regulating system. In the case of the failure of speech, the affective disorganization of the higher cognitive processes shows how profound the disintegration provoked by damage to the higher regulating systems may be. The reactive process of the adult human cannot be explained as a mechanical habit. It is constructed not only from below, but also from above, including within itself the regulating systems of a higher psychological order (ibid.).

Although “these systems may be disturbed during affect and conflict, they may also aid humans in overcoming disorganization” (396). Furthermore, one might add that finding the means of compensating for various disorders might also be the means of proving Luria’s ideas about the nature of higher psychological processes.

In a small way this is what Luria set out to do in **Chapter XII**, ‘The Control of Behaviour’. This final chapter begins by dismissing current theories on the nature of voluntary activity or ‘will’, mainly on the basis that they were too abstract to take account of psychological processes. They were either voluntaristic - even hedonistic - or mechanically reductionist. The supporters of the latter approach, who included Kurt Lewin with his theory of *Quasi-Beduerfnisse*, consider “the voluntary act as automatic, [claiming] that these automatisms direct the free ‘I’, and they posit that the executive of the automatisms is necessity, inclination, emotion” (399). “Nevertheless, the study of the *Quasi-Beduerfnisse* put us on the right track in the resolution of these complicated questions. In view of the fact that the ‘voluntary mechanism’ is a mechanism of subordination, especially by artificially created stimuli that may replace natural necessity, the theory makes an actual step forward in the scientific understanding of this problem” (400). This is evident in the tension created by the structure of an action that is interrupted before completion – as in the experiments of Ovsiankina and Zeigarnik.

[The difference in the use of auto-stimuli and quasi-needs is] what primarily distinguishes humans from animals, together with the fact that humans are able to control not only the external world, but also their own behaviour indirectly by the creation of artificial necessities and stimuli produced artificially especially for the purpose. This is a cardinal factor in the development of behaviour.

This cultural development is the means whereby humans may include a dynamic mechanism, which allows them to master their own behaviour and [also] bring about automatically the corresponding actions. While in the first stages of his development humans were able to act only on the surroundings, making instruments that helped them gain mastery over the external situation – his further growth began to elaborate those artificial stimuli that enabled them to think of themselves as objects of action and that aided them in controlling their own behaviour... *Voluntary behaviour is the ability to create stimuli and to subordinate them*; or in other words, to bring into being stimuli of a special order, directed to the organization of behaviour (401).

Luria then cites his works with Vygotsky as evidence that initially such control came from without in the form of the production of cultural stimuli. “This external auto-stimulation is substituted by an internal one. The ‘spontaneous’ establishment of complicated *Quasi-Beduerfnisse* seen in the adult are the result of the profound cultural reconstruction of activity dependent on the cortical apparatus, without which we could not understand complex psychological functions” (402). However in the experiments about to be discussed we should remember “the fundamental law: *direct attempts [by the subject] to control his behaviour always lead to negative results; its mastery is achieved only by indirect means*” (403).

This law is confirmed - notably in attempts by young children or hysterics to exert direct, unmediated control over their behaviour. Luria felt obliged to note the paradoxical nature of this ‘voluntary control’. This was shown firstly in an experiment with a five and a half-year-old child that took place in April 1930 (404-5), and then in experiments with hysterics (408-9). In the latter case Luria found that the use of indirect methods proved more successful. It was to be expected that both children and hysterics would react impulsively. Those suffering from Parkinson’s disease were affected by a disorder of the subcortical ganglia, but the cortical processes ought to operate relatively normally, and so ought to be able to compensate in appropriate circumstances. Luria’s combined method often met with too rigid a response. It was found that a subject not able to walk across the floor was able to climb stairs (Gantt has ‘ladder’), and that if pieces of paper

were placed on the floor as signals to stimulate each separate step, as on the stairs, the subject could then walk across the floor (410). (In later accounts, Luria (1979, 128-9, and E. Luria 1994, 72) attributes the discovery of this method to Vygotsky. Vygotsky also reports the experiment (Vygotsky 1997f, 105-6) and possibly implies the presence of I. D. Sapir). The problem lay in linking an appropriate stimulus or signal to a functional system. Luria found that the act of winking, a semi-voluntary activity, still functioned well and required minimal effort. So “this activity (winking) was made a conditioned signal in order to connect two reactions into a single functional system...” (Luria 1932a, 410). Speech has more significant functions, among which is

the means of regulating and organizing the external world by including the separate elements in a stable framework. On the other hand it is the agency for organizing behaviour, planning further action, and making it possible for the human to avoid complete reliance on direct optical situations... While some authors, such as Piaget, consider the speech of the young child to be ‘egocentric’, merely accompanying the activity of the child, we have proven that ‘egocentric’ speech has the function of planning the activity of the child and thus stimulating it. [Here the ‘we’ refers to Vygotsky]. This new planning role for speech ... begins with cases of auto-commands and ends with complex forms of judgment.

After this disturbances in behaviour associated with the failure of speech become clear. Namely, those facts showing that the primary defect in aphasia is one of ‘volition’: the inability to elaborate intentions, the dependence upon external situations, and the marked distractibility of the subject – are comprehensible when we study the central role that speech plays in the organization of human behaviour (Luria 1932a, 412).

In one experiment an hysteric proved able to control his movements by including auto-dictation – something that he was unable to do by direct, i.e., unmediated effort (413). Although Parkinson’s disease often proved intractable, if the speech connection was included within a larger structure, some success was obtained. That is, while a direct attempt to control behaviour failed, it succeeded when activated within a different system (414-6). “The process of the elaboration of the functional barrier consists in the transfer from direct action to a cultural, indirect

operation. The delay of the immediate impulses, the isolation of the excitation from a direct discharge into the motor sphere, and the turn to a preliminary, central preparation of the process preempts direct attempts” to act. “The inclusion of the reactive process within the ‘circuit’ of higher psychological systems constitutes the mechanism of the functional barrier” (420).

Finally Luria looks at the importance of the case where a patient is gradually drawn into making conscious a traumatic situation. The case was dealt with by Luria’s old colleague, the psychiatrist, analyst, historian, and sometime therapist for Adolf Ioffe, namely Iuri Kannabikh. The patient was hysterical. She suffered from an alcoholic and abusive husband. Under hypnosis she was eventually able to come to terms with her trauma by the mediation of drawing her terror in symbolic form, a picture which included a snake, a common symbol in Russia for drunkenness. Having produced this, she was then able to go on to control her emotions (423-6). “This experiment allows us to understand the mechanics underlying the action of symbols as emotional signals, and on the other hand, it helps us to see how the mechanism of substituting cultural symbols for the primitive process is the most important factor in the control of behaviour” (426). Perhaps Luria chose this example to bring us full circle in his experimental explorations. This time he could explain such a result both in detail and in terms of an overall scientific theory.

The development of the human as an historical subject occurs as the elaboration of historical, cultural behaviour. This development elicits certain new mechanisms that are peaks in his historical evolution. Speech and the use of signs, the permeation of activity by the use of cultural means [transform the human]. These new functions do not remain isolated psychological processes, but permeate the whole activity and structure of behaviour so that we find them literally in every finger movement. To understand human behaviour and its disintegration and organization without these cultural, psychological mechanisms is impossible... The analysis of complex cultural mechanisms is the key to understanding simple neurodynamic processes. We have done this with only a few psychophysiological processes, but we are convinced that this system of investigation answers many of the riddles of human psychology (428).

III

The Significance of the Development of the Concept of Functional Systems for Psychology

Of the six reviewers of the book that I have read from 1933-5, five wrote, often at great length, about the shortcomings of the translation. One, Lawrence Kubie, stated among many complaints that the book was “full of passages which are either partially or totally unintelligible” (Kubie 1933, 331). He attributed other shortcomings to Luria, including the use of undefined pseudo-physiological terminology, but this is impossible to justify without the Russian original. Another reviewer, J. F. Brown, who had worked in Germany with Kurt Lewin and was at that time publishing his own research in *Psychologische Forschung*, commented that Luria’s German papers “would certainly not convince one that he is an unbelievably clumsy stylist” (Brown 1933, 381). This I can confirm from both his German and Russian texts. Kubie criticized Luria’s presentation of experiments and methodology. It is quite clear from the text, however, that Luria is in many cases citing the research of others, and is also often using the examples in an illustrative way. In a book already 450 pages long a detailed account of every experiment was impossible. Kubie’s evident animus towards Luria may be based on an over-zealous defence of psychoanalytic tenets against Luria’s theoretical developments or sheer frustration at the text. He did, however, consider Chapter IV’s use of hypnotic suggestion to be “of real value to all who are interested in dynamic psychology and particularly perhaps to the psychoanalyst” (Kubie 1933, 334). He considered it confirmed fundamental psychoanalytic findings. The editor of the *Psychoanalytic Review*, on the other hand, omitted to mention this chapter altogether, but was much more favourable to the book as a whole, describing it as “this very significant contribution” (White 1934, 236).

This appears to have been the general response to the work. I did initially wonder whether the quality of the translation and Gantt’s ‘condensing’ of explanatory passages might have prevented the reviewers from grasping its key points, but although Kubie was not alone in some of his criticisms, only Taylor found Luria’s

theory “confused” and his accounts of experiments “sometimes puzzling” (1934-5, 232). Most noted Luria’s comments that he was neither a psychoanalyst nor a behaviourist and the important role of his method, and half saw the influence of Lewin. Myers mentioned the important role of language in child development and the concept of ‘neurodynamic age’ (1933, 1362), Taylor also noted the latter, the importance of the central mechanisms, and the construction of the reactive processes not only from *below* but also from *above* (1934-5, 231). Everyone commented on the important role played by the ‘functional barrier’ in Luria’s work, but none mentioned the functional system or the influence of Head. Geldard (1933, 487), Myers, White and Brown all considered the work to be an important scientific contribution, but only Brown showed any real insight into it.

Brown was already familiar with Luria’s German and English articles. He was very impressed. “In Luria’s work we have the beginnings of an experimental psychopathology. Its significance is therefore scarcely to be overrated” (1933, 377). “Primarily the significance of Luria’s work is a methodological one, although his factual contributions are by no means few or unimportant” (378).

Before he was able to devise [the combined verbal and motor response] method, however, he was forced to consider the theory of organized behaviour. Before he was able to set up his working hypothesis he was forced to define precisely the concepts with which he wished to operate. His general mode of attack is what Lewin calls the ‘constructive’ method... [This comprises three steps:] (1) a precise definition of concepts ... , (2) the manufacture of a working hypothesis, (3) its experimental verification or invalidation. Of the hundreds of workers on emotion, Lewin and Luria have been the only two to use this method consistently and have hence, in my estimation at least, made the most important contributions of contemporary psychologists to our knowledge of emotion (ibid.).

Although Khomskaia considered Luria’s planning of experiments to be similar to Anokhin’s approach (Homs kaya 2001, 115; cf. II, ii, 58), I think Brown has a relevant point here. He adds, “In using this method Luria first defines his concepts in terms of a theory of organization” (1933, 278). This is true and exceedingly important in view of the various reductionist approaches that did not even look at

organization. Undoubtedly also Gestalt theory helped Luria enormously in this. But, contrary to Brown, not only did he not use the ‘configurational’ approach, but he went beyond Gestalt theory. Brown adds another important point, namely that “Luria’s greatest contribution seems to me to be in the application of the ‘constructive’ method to the psychology of emotion... What we need in psychology is more rather than less theory. On this point Luria’s work is convincing... [He] uncovers an impressive array of facts and the beginnings of some laws about the dynamics of conflict” (ibid. 379).

Luria “develops a theory of the ‘functional barrier’ which receives confirmation in his researches on the ontogenetic development of organized behaviour, on hysterical patients and on the mentally deficient” (ibid.). Here Brown seems to be going along with Luria on the importance of development – further than his Gestalt colleagues are often presented as going. He cites Luria on the importance of complex cultural mechanisms as the key to understanding simple dynamic processes, and agrees with Luria that “this system of investigation answers many of the riddles of human psychology”. Although he does not formally tie the two points together, he also quotes Luria, “Psychology finds itself in a great crisis, and what had been formerly studied apart as independent isolated activities are not investigated in their functional relations to one another” (ibid. 380, cf. Luria 1932a, 428).

Brown at least could see where Luria was going. But he never mentions ‘functional systems’. While I do have the luxury of hindsight and a great deal more space than Luria’s reviewers, I do think it is legitimate to conclude from Luria’s book that logically, for him, the concept of the functional barrier, the ontogenetic development of higher psychological processes, and the transformation of a psychology of isolated studies into an integrated science do, in a sense, depend upon the concept of the functional system, especially as applied to higher psychological processes. It is, however, true that the connections do need to be spelled out. It is also true that Gantt admitted that he had condensed the theoretical discussion (cf. V, introduction), so that we do not have Luria’s complete views on the functional barrier and the functional system. Indeed the additional material in the new Russian edition of the book will almost certainly

confirm this and help elaborate this key area. I will now attempt to draw out the theoretical implications of these concepts from the presently available material.

Today many people will be familiar with Luria's association with the term functional system, but few will be familiar with the term functional barrier. Indeed the latter term does not seem to appear at all in Luria's later works. Although much of his postwar work concerned aphasia and other neurological disturbances, he did not find it necessary to employ this concept. Perhaps the term was more closely related to thinking about *neurotic* disorders. In a sense the incorporation of cultural mechanisms in this barrier might be extended to cultural meanings, cultural mores and ideologies. Perhaps when W. H. R. Rivers criticized Freud's use of the 'political' concept of censorship in a neuropsychological context, Luria felt that a term like functional barrier might be a suitably scientific reinterpretation. But there is no evidence for this. Perhaps Luria was worried that it might indeed be interpreted in this way, because it only seems to have real value in a psychiatric sense, and then only as a metaphor. Perhaps the theoretical commentary about to be published in Russian but 'condensed' in English will clarify this.

However, even in *The Nature of Human Conflicts*, Luria formulates an alternative solution - namely, that the receptory-connecting system and the motor effector system "play functionally unequal roles in the activity of the organism, and they control incomparable structures" (Luria 1932a, 289). As I said in my commentary (cf. VII, i, 187), this does not mean that Luria viewed the two approaches as incompatible. Perhaps, rather, he subsequently saw the concept of the functional barrier as something of a distraction from the main points he was trying to put over. After all, the higher psychological processes *as a whole* were formed with the incorporation of the same cultural mechanisms that were found in the concept of the functional barrier. As I suggested earlier, the concept of barrier owed much to Kurt Lewin and, though it made sense in terms of his concepts of structures and fields, perhaps Luria felt it was a distraction in terms of his emphasis on systems and processes. To have one's theory misunderstood on the basis of an additional and, I would say, marginal and, in the final analysis, logically unnecessary concept would not reflect good scientific practice. It may also be the case that, in

the hostile climate that Luria faced from his involvement with cultural-historical theory, he felt he should cut his losses and jettison the concept. On the whole I prefer the ‘scientific’ argument for Luria made no attempt to resurrect the concept, whereas in the case of the functional systems approach, he maintained its use throughout his career, even though pretending it was the physiological version of the concept proposed by his friend Anokhin.

As previously stated, the functional system appeared in its debut as a *Funktionssystem* or system of functions. Luria did not connect it with Goldstein or Lewin, but related it instead to Hughlings Jackson and Head. It seems that Luria came up with the term independently of them all and in relation to his own ‘prototypical’ model of the combined verbal and motor system. He saw how this system operated when joined and when in a state of disintegration. Inevitably when he read Head’s book on aphasia, he recognized the systemic nature of the various disorders. This not only provided a key to the structural organization of the higher mental processes that he was studying, but elicited a stream of publications from Luria about aphasia that continued for the rest of his career. Head wrote, “A want of chronological exactitude [in cases of aphasia] will throw the whole movement into disorder; its ‘kinetic melody’ has been destroyed” (Head 1926, I, 88-9). [NB. Head *may* have borrowed the term ‘kinetic melody’ from the early Gestalt theorist Christian von Ehrenfels’ (1859-1932) studies on music – see his articles in Smith 1988]. Luria responded warmly to this term and often used it in his later writings. And it is the creation of these *working* systems that underlie not only the operation of the higher mental processes, but their very existence. In accepting that new psychological systems can be created out of already existing morphological formations in the brain, together with external stimuli, including the mediation of various cultural instruments, including languages, all the limitations of holistic and localizationist approaches to the operation of the brain became redundant. It is the comprehensive nature of this theoretical transformation that is so striking.

The functional systems approach is also non-reductionist in that it can accommodate many other forms of partial or apparently discrete systems in its overall approach, especially in lower physiological systems. It is not a theory as

such, which thereby allows it to be more inclusive. It is not initially prescriptive about the nature of interrelations and interactions between such systems – that would require a theory. Certainly the relationships between many systems were already apparent at some level. Furthermore the Spencerian concept of ‘levels of integration’ as mediated by Jackson and received by Anokhin (1974, 190), and discussed in the previous section of this chapter, while not explicit in Luria’s book, is certainly implicit and only awaiting the republication of Jackson’s work for it to be made explicit. As it stands at this time, however, it is rather an explanatory approach that facilitates research into the nature and operation of such systems as a necessary prerequisite for such higher order theorizing. But in World War II it fell to Luria to follow in the steps of Kurt Goldstein and Henry Head and diagnose and treat those who had suffered war wounds; his approach was *already* of immense practical use.

It is true that much of this is implicit in Lewin. The researches of Lewin and Luria confirmed that one could not achieve anything, including a degree of self-mastery, by a simple direct voluntary act, but that it was mediated and indirect. Although speech itself is considered to be a fairly direct form of action in comparison with other forms of signalling or self-signalling that Luria employed, such as winking or placing sheets of paper across the floor, it also has many components that in turn may be party to many different functional systems. This fact is now well known to students of aphasia. The point is that even apparently direct activities are themselves mediated in many ways.

Although both Lewin and Luria accepted this, Luria felt that Lewin adopted an automatic and determinist approach to the solution of needs. He did grasp that humans possessed this form of organization, which meant that both the bottom up and top down systems combined in quasi-needs. Yet he failed to accept a possible consequence of this, namely, that if humans could operate from the top down in order to meet those needs, they could consciously formulate their own needs and, to a degree, take charge of their own destiny. Although Luria introduced this important element into *The Nature of Human Conflicts*, he does not expound on it at any great length. This was a major contribution of Vygotsky, following Herder, Marx et al. Surprisingly Vygotsky barely appears in the book. Perhaps this is

explained by Luciano Mecacci in his survey of Russian neuropsychology and psychology, *Brain and History*. Although Vygotsky was a strong exponent of the position that there was a cerebral reorganization of higher psychological functions and did pursue studies of his own, his ideas “could not be adequately developed by him. However the idea of a ‘growth’ of cerebral functions in the course of ontogenetic development and the possibility of delineating their real ‘history’, strictly connected to the individual history, remained fundamental. It should be noted that Vygotsky’s conception does not concern the concrete ‘way’ in which the functional connections of the brain come about, this being to him the territory of physiological research” (Mecacci 1979, 63). This confirms my view that whatever Vygotsky’s theoretical contribution, which was surely considerable, and also inspired by the work of Goldstein, Lewin, Head, and Jackson, Luria’s complementary contribution was vital in order to ground Vygotsky’s work in terms of neuropsychology.

In Luria’s preface to Mecacci’s book, probably one of the last things he wrote, he does praise the contribution of Anokhin and Bernshtein to the development of physiological theory and the role of the concept of the functional system in their work, but he does not attribute his own ideas and theoretical development at this stage (1932) to them. This is entirely comprehensible since we have seen that Anokhin’s own account states that he did not develop his ideas till a little later. Instead, while Luria again fails to discuss his own role, he praises that of Vygotsky and, at the end of this passage, that of Leont’ev. He writes:

Without denying that every type of behaviour can be broken down into elementary reflexes, Vygotsky recognized behavioural ‘units’ not in the reflexes themselves, but in those complex forms of mediated activity that arise in society and in history and that constitute the essential components of complex human mental activity. It is apparent that this point of view would lead to a revision of the principal conceptions of mental activity. This is why Vygotsky, from the outset, concentrated on those complex and changing *functional systems* of cerebral activity that are formed in the historical-social process. He called these systems ‘extra-cortical functions’ and then came to include in the natural processes of cerebral activity such things as external supports, objects in the external world, helping

means, and, above all, language. These represented the cerebral work that is carried on by means that are objective and external to the brain, and that had been created in the course of the history of human society. Vygotsky's students and one of those who continued his work, Alexei N. Leont'ev, used to call them 'functional organs', meaning that the brain, in its process of development, confronted new tasks not so much by forming new morphological organs but rather by creating new and changeable functional systems (Luria 1979b, xiii, cf. also Luria 1979a, 124ff).

At this stage one begins to wonder whether Luria suffered from some sort of delusion, a 'Luria syndrome', of being pathologically unable to present his role in any process of discovery that also involves Vygotsky. Vygotsky's role in the founding of cultural-historical theory remains unquestioned, but to see neuropsychology and functional systems entirely in these terms is completely one-sided. No wonder several serious scholars attribute the developing use of the concept of the functional system to Vygotsky. Yet we have seen how the 'system of functions' developed from Luria's combined verbal and motor response method. He never mentions his collaborator Lebedinskii in respect of the term. Lebedinskii's own book does not use the term (Lebedinskii 1931); nor was it used in an earlier joint article (Lebedinskii and Luria c.1929) that was the Russian version of the German article in which the term first appeared. We have also seen that at the time Luria did not mention Vygotsky in this respect, but rather Jackson and Head.

The long-running confusion caused by the 'Luria syndrome' is well documented. Levitin writes, "Luria shunned publicity and avoided answering [Michael] Cole's question" of how he came to be the best known Russian psychologist in America. He later complained to Levitin "over the telephone that his name had [appeared in an article] ranked with those of the great scientists Pavlov and Sherrington" (Levitin 1982, 134, also 165). Although he was willing to talk about concrete studies and other scientists, he presumably felt it was not his role to express his own specific contribution or to judge his own significance – and when others did he became embarrassed. But finally, as a form of postscript to the discussion of this issue, we must accept that the most significant factor in this 'Luria syndrome' was not a personal problem, but, as Michael Cole very recently pointed out (2002,

personal communication), simply *the fear of having a high public profile* that Stalin engendered in generations of Russians.

Since most Western writing focuses on Vygotsky, Luria's praise of him is simply taken for granted. Luria said, "The most exciting years of the century and of my own life were the twenties – those associated with Vygotsky. I can hardly claim any credit for what I've done" (ibid. 164). We cannot leave such statements unaddressed. As regards cultural-historical theory we should ask instead why Vygotsky chose *Luria* as his principal partner. Previously he had also encouraged Luria's independent work, particularly in the development of what became a whole new approach to neuropsychology – and was rewarded with what in effect provided the neuropsychological 'ground' of cultural-historical theory. When Luria says how Vygotsky encouraged him to use the combined motor and verbal method in investigating the role of speech in the organization of voluntary activity and planning, we should ask why (cf. Luria, cited in E. Luria 1994, 43, Luria 1979a, 51-2). Luria may not have been prepared to face this issue, and many writers have failed even to recognize it. But *Vygotsky* certainly recognized the significance of Luria's work, both his independent work and their joint work, and he knew that without Luria he would not have achieved half of what he set out to do. On his part Luria certainly felt the same. Together they were more effective, stimulating and significant scientists than on their own, which is why Luria looked back so often to their time together. I think this is the only realistic and comprehensive way of understanding their relationship.

Oddly enough, Vygotsky shared a common feature with Head and Jackson: he never used the term functional system – except in one article which he co-wrote with Luria, to which we will now turn. *Tool and Sign in Child Development* was a long work apparently intended for publication in Carl Murchison's *Handbook of Child Psychology* in 1930. It is not clear whether it was completed in time, nor why it was not published in full until the 1980s (Vygotsky and Luria 1994b, 170 notes). It was nevertheless written about the time that Luria was completing *The Nature of Human Conflicts*. The style of the writing is certainly that of Vygotsky, but the use of the term functional system several times in the conclusion appears to confirm the presence of Luria as co-author. But it is clearly Vygotsky's agenda

and literary approach that drive the piece, which is not only interesting, but important in that it raises issues that will be dealt with at greater length in the following chapter.

Part 6 of *Tool and Sign*, ‘Conclusions’, comprises three sections. In the first, ‘The Problem of Functional Systems’, they write:

As our studies show, not only is there an internal reconstruction and improvement of separate functions [in the process of the child’s psychological development], but intra-functional connections and relations change in a radical way. As a result new psychological systems appear that unite in complex co-operation a number of separate elementary functions. For want of a better term we call these psychological systems, these units of a higher order that replace homogeneous, single, elementary functions, the *higher psychological functions* (Vygotsky and Luria 1994b, 162).

Given the *modus operandi* of these systems we must “acknowledge *the unity, but not the identity, of higher and lower psychological functions*” (ibid. 163).

In the second section, ‘The Use of Tools by Animal and Human’, they add:

The higher form of activity is present wherever there is a mastery of one’s own behavioural processes, and initially one’s reactive functions. In subjecting the process of his own responses to his will, man thereby enters into a substantially new relationship with the environment, comes to a new functional use of environmental elements as stimuli-signs that he uses as external means to guide and regulate his own behaviour... Internal regulation of purposeful activity arises initially in external regulation. Reactive activity elicited and organized by man himself ceases to be reactive and becomes goal-directed. In this sense, the phylogenetic history of man’s practical intellect is closely linked, not only to mastering nature, but also to mastering himself. The history of work and the history of speech can scarcely be understood without the other (Vygotsky and Luria 1999, 63).

“The development of [this] *freedom of action*, as we have tried to show [in the previous parts], is in direct functional dependence on the use of signs” (ibid. 65). Whereas the quotations from the first part are totally consistent with Luria’s work, this second set of quotations, though implicit within it, clearly shows the influence of Vygotsky’s development of language theory in its application to child development and learning processes. It is a significant complement to Luria’s research in that the linking of the two enables us to begin to see psychology as a unified science, joining physiology and neurology to the development of both cognitive abilities and voluntary behaviour. This is related in turn to the mastery of nature, a historical conception developed by Marx. Although there may always be semi-discrete areas of study within psychology and associated sciences, the possibility of a unified science is again made apparent, this time more from Vygotsky’s perspective.

In the final section, ‘Word and Act’, they recognize “that all of the history of higher psychological functions is nothing but a change in initial functional relations and connections and the appearance and development of new mental functional systems” (ibid.). The *‘novaia psicheskaja funktsional’naia sistema’* is now, unsurprisingly applied to that to which Luria first applied it, namely “the interfunctional relation of word and act” (ibid.).

[The studies of Luria and Vygotsky led them] to the conviction that there cannot be a singular formula that would encompass the whole range of variability of the relations between speech and action at all stages of development and in all forms of disintegration. In truth, the dialectical character of the development of functional systems cannot be adequately reflected in any one constructive formal, logical scheme ... [because none of them] considers the movement of concepts and the processes that lie behind them, the changeability of relations, the dynamics and dialectics of development (ibid. 66).

The most remarkable part of what happens with action and word in the process of development is ... the development of egocentric speech and ‘thou-centric’ action [i.e., speaking and acting as if to another], the conversion of a social method of behaviour into a function of individual adaptation, an internal transformation of action with the help of the word, [and the recognition of] the social nature of all

higher mental functions, including practical action in its higher forms... This new relation of action to the individual, which arises due to the word and leads to mastery of action, this new relation of the actor to the external world, is manifested in free action, controlled and directed by the word – none of this arises at the beginning of the process of development, and, for this reason, is not [normally] taken into account at all (ibid. 67).

Finally, “we might say: if the act, independent of the word, stands at the beginning of development, then at its end stands the word, which becomes the act. The word, which makes man’s actions free” (ibid. 68). This gloss on Goethe’s *Faust* is a typically Vygotskian literary flourish. Similar presentations almost certainly contributed to those ignorant commentators who dismissed him as not really being a psychologist. Yet not only has Goethe’s remark “in the beginning was the deed” been directly linked to his onetime teacher, Herder (cf. Schuetze 1925, 546), but Herder also recognized that reason was not innate, but arose only in the course of human development, involving both historical and ontogenetic factors (cf. Appendix I).

Inevitably, in the circumstances of the early 1930s (cf. VIII, i), Vygotsky was criticized for seeing psychology in individual terms. But without a theory of internalization, its social origin and cognitive, educational, intellectual, social and emotional consequences, not only do we lack the rudiments of a psychology, but the alternative theory of the individual as a sausage skin filled with social content – all too common in Russia in the 1920s - is not only reductionist, but leaves the concepts of both society and the individual totally unarticulated. Those who attacked Vygotsky in the name of society were obviously unaware of Marx’s own stated position, namely that, “We must avoid postulating again ‘society’ as an abstraction vis-à-vis the individual. The individual *is the social being*” (Marx 1975, 298-9). Luria’s version of their joint comments in *The Nature of Human Conflicts* (Chapter IX, 301-3) says much the same thing, but in very much ‘safer’ terms. Vygotsky and Luria were also criticized for introducing the concept of the sign into the discussion in an idealist fashion. We shall examine this aspect in the following chapter.

Between 1932 and World War II Luria published only two papers directly related to his work in *The Nature of Human Conflicts*. These were ‘Psychology and the Theory of Localization’ and ‘Problems of the Development and Disintegration of Higher Psychological Functions’ (Luria 1933a and 1933b). As yet I have not succeeded in obtaining a copy of either. Thus we do not know how he developed this side of his theory in the 1930s. Vygotsky also wrote two papers on identical subjects. Luria was at that time forced to work in Kharkov and Vygotsky hoped it might be possible to join him there and work together in the field of clinical psychology, but this move did not happen (E. Luria 1994, 72). Both Luria’s papers were delivered in the Ukraine, as were Vygotsky’s. Vygotsky did not deliver them himself as he was dying. ‘The Problem of the Development and Disintegration of Higher Mental Functions’ (Vygotsky 1960) has not been translated into English, though it is available in Italian (in Mecacci 1976, 330-47). The other paper ‘Psychology and the Theory of the Localization of Mental Functions’ exists in two translations, one by Luria (Vygotsky 1965), and one in the *Collected Works* (Vygotsky 1997g). It is a major work. In a few pages it brings together the threads of several arguments in *The Nature of Human Conflicts* – both explicit and implicit - and crystallizes them quite neatly. When Luria rescued it from obscurity he ensured its publication, translated it and accompanied it with his own article. It would be interesting to compare his own paper from 1933 on the subject, but that is not at present an option.

Localization is usually a subject closely tied to the diagnosis and treatment of lesions to the brain. In the nineteenth century famous discoveries by Carl von Wernicke and Paul Broca had uncovered areas of the brain associated with speech and its disorders, specifically sensory and motor aphasia. However in general such disorders proved to be more systemic and Goldstein and Lashley thought that the brain acted in a more holistic fashion. Vygotsky (1965, 381) argued that “the problem of localization is in essence the problem of the relation of the structural and the functional units in brain activities”. Although Gestalt psychology appeared “to be very productive in the early critical part of [its analysis by] overcoming the atomistic approach” (ibid.), it did not address the issue of the functional units.

An adequate approach to the localization of functions can [however] be built on the basis of a historical theory of the higher psychological functions, with the basic idea that the higher mental processes are meaningful functional systems. The leading assumptions of this approach are: (a) the assumption of plastic, changeable interfunctional relations; (b) the assumption of complex dynamic systems which have to be considered as the result of the integration of elementary functions; and (c) the assumption of a categorical reflection of reality in the human mind. All these basic assumptions reflect the most important features of man's psychological features. They are an important example of the dialectical leaps in the transition from perception to conceptual cognition, which is as basic as the transition from the inorganic to the organic forms of existence (ibid. 382).

I have chosen Luria's translation on the basis that his version draws out better the significant phrases and issues from what is described as a summary of Vygotsky's conference paper. It has to be admitted, though, that in the original (cf. Vygotsky 1982, 169) the term '*functional system*' does not appear. On the other hand, the addition of 'functional' does improve the sense, especially in terms of Vygotsky's initial statement.

[A comparison of the effects of local lesions in children and adults] led us to the conclusion that identical syndromes *in both cases can be a result of the different localization of lesions and vice versa that lesions with identical localization in children and adults can result in very different disorders*. We can formulate a law for these basic differences. In disturbances occurring in early stages of development resulting from a local brain lesion, it is the nearest *higher* centre that suffers the most, whereas the nearest lower centre suffers less. In local brain lesions to a mature brain it is the nearest *lower* centre that functionally depends on the higher zone that suffers primarily, whereas the nearest higher centre, which became independent in the course of development and which functions at a higher regulatory level, suffers less (Vygotsky 1965, 384). An explanation of these data can be found in the basic fact that *the complex interrelations of different cortical zones are the result of development*, and that *different interrelations exist at early and late stages of development* of a human being. 'Lower' levels are basic for the development of the 'higher' levels, and it can be easily proved at the early stages of development. But as a result of the general law of a shifting of functions towards the highest level, these 'higher' levels become independent in the cause of further

ontogenetic development. Development goes upward, dissolution downward. Some additional proofs come from the observation of *compensatory mechanisms* in cases of local defects. In the mature brain these compensatory functions are performed by the higher ‘centres’, in the earlier stages of development, by the ‘centres’ lower than the injured zone. That is why a comparative study of development and dissolution is one of the most fruitful methods of the analysis of localization of function in general and especially of the problem of so-called chronogenic localization (ibid. 385).

Clearly you do not have to be an intellectual ‘apparatchik’ to understand the value of ‘materialist dialectics’ here. Interestingly Luria renders the Russian for ‘development’ and ‘disintegration’ into the Jacksonian terms ‘evolution’ and ‘dissolution’, and quite appropriately too (cf. Vygotsky 1982, 173).

In the final quotation we present here Vygotsky reports on a study of aphasia, agnosia and apraxia. He concludes (1965, 385):

‘Extra-cerebral’ connections play an important role in the localization of functions in these areas. These ‘*extra-cerebral*’ connections are basic for the functioning of speech, cognition and action in the normal person, and their disturbance results in the syndromes mentioned above. We come to this conclusion after a series of observations of the course of development of higher forms of psychological processes. These observations showed that in the first stages these functions are intimately connected with external activities, and only in the later stages of development do they become ‘interiorized’ [and convert] to ‘inner mental activities’. The same can be seen in the observation of the [source] of compensation for functions disturbed by local brain injuries. It was seen that an ‘exteriorization’ of the functions and their connection with some external objects as instruments is one of the most efficient ways for their compensation.

With this confirmation of the later experiments in Luria’s book, and the clear connection with the means of social and historical activity (marked by Luria’s footnote about tools and signs in his commentary) we will now turn to the origins and development of cultural-historical theory.

PART III

CULTURAL-HISTORICAL THEORY

Chapter 8: Cultural-Historical Theory as a Developmental Approach; Epilogue

I

The Growth of Stalinism

As a result of World War I and the invasions and interventions of Western governments in the Russian Civil War of 1918-21 the Russian economy was put back decades. The results were twofold. Firstly the major cities were depopulated as the workers returned to the countryside to search for food. This meant that the government lost its necessary popular-democratic base and decisions were made by a small number of people. This lack of accountability facilitated the rise of governmental cliques, notably that of Stalin. Secondly, the government was forced to adopt the New Economic Policy (N.E.P.), which tried to encourage the peasant market economy and get the country back on its feet. After Lenin's death in 1924, Trotsky argued that the balance should be altered so that industry should dominate over agriculture, and the working class, the backbone of the party, should be rebuilt. This decision was postponed until the end of the twenties. By this time Stalin had sent Trotsky into exile, expelled him from the Party and in 1929 expelled him from the Soviet Union. Effective opposition was thereby stilled, but Stalin did not yet have the confidence to have Trotsky killed.

In 1929 Stalin introduced the first Five-Year Plan to industrialize Russia at a breakneck pace. This was not what Trotsky had argued for. Stalin had opted to use the state to implement a form of primitive accumulation of capital, as had occurred in Britain in the early nineteenth century. He followed British practice by starving the peasantry and driving them off the land and into the cities. There, as workers, they were ruthlessly exploited to an extent surpassing the horrors of the period of the primitive accumulation of capital in the West. This was a full-scale counter-revolution. The workers were denied all rights, thus ensuring that there was no basis for a democratic socialist party and that Russia would be ruled by various cliques for the rest of the century. As justification for this rapid industrialization Stalin cited the danger of another Western invasion – for which no real basis in fact existed at that time. He instituted a state of siege mentality,

staged show trials of foreign engineers to demonstrate the real danger from outside, and interned Trotskyists to stem the alleged danger from internal agitators. This state of siege mentality proved crucial in preventing opposition to his implementation of state capitalism. It also intimidated the *intelligentsia*, who were afraid of being accused of cosmopolitanism.

Stalin was not interested in improving working conditions or understanding theories of educational development. What he wanted for the economy was basic training for large numbers of unskilled workers, together with extra training for skilled workers. He had no use for psychological theories or psychologists. By the end of 1936 psychology had been largely eliminated as an independent academic discipline. In the nineteen twenties Stalin adopted Bukharin's slogan of 'Socialism in One Country', a contradiction in terms for any Marxist, and in 1936 he declared Russia had achieved socialism. As one celebration of this in 1938 he had published *A Short History of the Communist Party of the Soviet Union (Bolsheviks)* in which he notoriously changed the philosophy of Marxism. It was perverse of Stalin to continue to promote himself as a Marxist, when he was in fact a Russian chauvinist who treated the workers worse than did self-confessed capitalists, exterminated the 'Old Bolsheviks', and anyone else whom he deemed a potential threat. Bekhterev had been overheard describing Stalin as paranoid in 1927 and was allegedly secretly poisoned by Stalin's agents within hours (Moroz 1989). Stalin had real reason to be paranoid now. The sectarian divisions he had inflicted on the international workers' movement had split the left in Germany and could be seen as the primary reason why Hitler had been able to gain power. Now Hitler did indeed represent a threat to Russia, and since Stalin was culpable of this in addition to his crimes against the people and the collapse of agriculture due to his policies, he could only govern by terror. This transition between 1929 and 1937 is now relatively clear, but it was extremely confusing at the time. For those Marxists attempting to create a unified approach to psychology it was a veritable minefield to negotiate. Literally the only way to avoid trouble was if no one was aware of your existence.

The famous 'troika' of Vygotsky, Luria and Leont'ev met first at the Institute of Psychology to organize their research into child development. Later they were

joined by the ‘five’: L. I. Bozhevich, R. E. Levina, N. G. Morozova, L. S. Slavina and Alexander Zaporozhets at Vygotsky’s apartment (cf. Luria 1979a, 45-50). Later they worked at the Krupskaja Academy of Communist Education where Luria was director of the psychology department. There they “set up an experimental laboratory ... to deal with pictography, i.e., a method of studying what Vygotsky called indicative activity – the mental processes whereby signs, tools and instruments are invented”. The team investigated how these aided the cognitive development of primary and secondary school children (Levitin 1982, 162). Unfortunately there is no space here to discuss this celebrated experimental team, its other members, and the famous experiments that were reported in Vygotsky’s publications. So this is probably the first study of Luria to omit discussion of the work of this team. But there are plenty of such discussions in the books on Vygotsky and, more significantly, the evidence is clearly to be found in Vygotsky’s major works, especially *Thinking and Speech* (Vygotsky 1986). This team was ordered to be broken up in 1931 and Luria, Leont’ev, Bozhevich and Zaporozhets moved to the Psycho-Neurological Academy in Kharkov in December 1931 (E. Luria 1994, 69). Vygotsky also worked there periodically, while Luria also continued to work in Moscow until the Institute closed his laboratory. When Vygotsky’s educational theories and its academic supporters were vilified in the late 1930s, Luria had already moved into the world of neurology and he was not mentioned. On the other hand, when the cultural-historical theory was attacked in the early 1930s, Luria and Vygotsky were presented as its joint creators (cf. the translations in Van der Veer (ed.) 2000).

The Academy of Communist Education was also named the Krupskaja Academy after Lenin’s widow, Nadezhda Krupskaja, who ran it for most of the twenties in an enlightened way. Luria seems to have worked there as well as at the Institute and the Psychoanalytic Society almost from the time he arrived in Moscow. He later said that he was pleasantly surprised when, as a young lad of 22 without party affiliation, he was appointed head of the laboratory, and also as director of the sub-faculty of psychology there, at a time when it normally only accepted party activists (cited in E. Luria 1994, 35). Although we have seen Luria’s interest in the issues of biological, historical and ontogenetic development there is no way that cultural-historical theory could have been developed without Vygotsky’s

knowledge and commitment to the writings of Marx and Engels. “The only member of the Institute with a solid Marxist background was Lev Vygotsky”, said Leont’ev (Levitin 1982, 116). Luria confirms that “Vygotsky was ... the leading Marxist theoretician among us” and cites Vygotsky’s early use of Marx’s comparison of the architect and the bee. He adds, “In Vygotsky’s hands, Marx’s methods of analysis did serve a vital role in shaping our course. Under Marx’s influence, Vygotsky concluded that the origins of higher forms of conscious behaviour were to be found in the individual’s social relations with the external world” (Luria 1979a, 43). Not, however, in the sense of being a passive reflection of the external world, as many thought. Indeed the use of the signs, tools and implements in inter- and intrapersonal relationships, mentioned in the previous paragraph, was to be a key element in both the area of child development and cultural-historical theory.

Luria’s comment on Vygotsky’s thoughtful and creative use of Marxism is also important to distinguish from the misuse made of Marx by his opponents. In the appendix I have explained how many of Marx’s ideas on language, human nature, consciousness and even dialectics were taken from or absorbed from Herder. Although this relationship has not been made explicit till now, Vygotsky was able to read Marx in something like a ‘Herderian’ way, because of his lifelong interest in the works of Potebnia. Vygotsky appears to have had no direct knowledge of Herder’s writings, but Potebnia, as the man who introduced Humboldt’s language theory to Russia, undoubtedly passed on some of Herder’s ideas, though in a somewhat different mixture.

As I have already pointed out, in his translated article of 1930, Kornilov used the same Marxist sources as Luria had in *Psychoanalysis as a System of Monistic Psychology*, i.e., the sources were accessible to all and used by all. But the interpretations were different. When Vygotsky broached his views on cultural-historical or ‘instrumental’ theory at the Institute he was attacked and ridiculed by Kornilov (E. Luria 1994, 42). Fortunately Luria’s grasp of Marxism improved over this period. Although in later years regarded as a ‘good party man’, who could be relied upon to present the current line, this was simply a necessary condition of existence in becoming or remaining a prominent scientist. This was a

point he made to Elkhonon Goldberg in his attempt to assist the latter's career in Russian psychology (Goldberg 2001, chapter 2). But he was also well aware of genuine Marxist theory as opposed to varieties of political propaganda on behalf of a state-capitalist Russia. Apparently he made pencilled comments in his Russian edition of the *Short History of the CPSU (B)* to the effect that it, i.e., Stalin, had got at least some of it wrong (Rawles 1998, personal communication). Thus when Vygotsky and then Luria began to theoretically develop and undertake research in the field of cultural-historical theory, they knew what they were doing in terms of Marxism. Cultural-historical theory was not an attempt to adapt psychology to Marxism in a verbal way, as sometimes happened at the time. Although it might be presented simply as a theory about cognitive development, it was an attempt to apply developmental approaches to psychology in general. Thus the emphasis of Marx and Engels on both materialism and the dialectics of change were what drew Vygotsky to view them as crucial for this attempt. Although he had always been a Marxist, it was in his works of 1928-30 that one finds most of his Marxist citations.

II

Cultural-Historical Theory as a Developmental Approach

Psychologists had already accepted the need to examine fields other than child development from developmental perspectives. Comparisons abounded between the thinking and speech of children, adults with schizophrenia or forms of aphasia, and so-called 'natural' peoples, and how they were all distinguished from non-speaking animals and from educated Westerners. The relationship between these cognitive 'levels' was confused. Common explanations included that of the biogenetic law, and theories containing partial versions of it. Another overall explanation, that development reflected a gradual move from diffuse forms of behavioural organization to a differentiation into more specialized structures, was often presented abstractly and without research. From 1927 onwards Vygotsky and Luria systematically reviewed and criticized these various proposals.

In an article on *The Biogenetic Law in Psychology and Pedagogy* Vygotsky criticized not only the attempts to introduce Haeckel's recapitulationist law into

psychology by Freud's maverick American supporter, G. Stanley Hall, but Wilhelm Stern's attempt to distinguish six phases in childhood that allegedly corresponded to six stages of cultural-historical development. The successive phases of reflex activity/ the ability to grasp and imitate/ upright gait and the use of speech/ play and fairy tales/first school years/ middle school years/ and semi-maturity allegedly corresponded to lowest animal/ higher apes/ entrance into human history/ antiquity/ Christianity/ and the present-day. Vygotsky criticized Claparede, Koffka and Blonskii for not recognizing that their acceptance of onto- and phylogenetic correspondence implied acceptance of an immanent logic in the process of development itself. It was true that this theory, together with that of sociogenesis, associated with Zalkind, Kornilov and A. P. Pinkevich were an improvement in that their adherents attempted to operate scientifically. But nevertheless these latter approaches made an almost identical mistake as the other approaches – an unjustified and scientifically one-sided move to attribute universal significance to biogenetic parallelism. Vygotsky considered that any attempt to apply them to education would be fraught with danger. Moreover attempting to apply the biogenetic law to pedagogy would be deeply reactionary (Vygotsky 1927).

In a review of Heinz Werner's major work of 1926, *Einfuehrung in die Entwicklungspsychologie* (Introduction to Developmental Psychology) Luria echoed Vygotsky's views on the biogenetic law. Luria noted how immensely complicated was the investigation of the constituents of human development involving as it did biogenesis, historical development and ontogenesis. Werner pursued an approach that proposed increasing differentiation and restructuring in his examination of the animal, the 'natural' human and the child, to which he added a fourth area of comparison, namely mental illness.

In this totally interesting and valuable book, certain fundamental deficiencies are, however, evident in his material, characteristic of most general philosophical-psychological subjects considered in West European writing. Would such a broad general principle as 'structural differentiation' really be sufficient to elucidate all special qualities of psychic development? Would it not thereby leave out of consideration a whole number of concrete conditions that determine the transition

from one form of behaviour to another? [In short the answer was that] a materialistic investigation can in no way be satisfied with such general assertions that are already known to us from [the works of Herbert] Spencer and the Spencerians. Within these general determinations the concrete circumstances must also be established that determine psychic evolution more precisely (Luria 1929b, 483).

Luria specifically criticizes the presentation of the psyche of ‘natural’ peoples as detached from their real life, their “being”. In later years Vygotsky and Luria were accused under Stalinism of uncritical borrowing from Western sources and eclecticism (cf. the articles in Van der Veer 2000). Recently even, one Western writer accused Vygotsky and Luria of having endorsed the biogenetic law (Joravsky 1989, 369). Yet others have suggested that cultural-historical theory was a form of reductionism, allegedly Marxist, that connected psychological development and thinking to the mode of production of a given society. But we have seen that a philosophically similar position is actually criticized above, under the term ‘sociogenesis’, for its one-sided approach.

We can see that both Vygotsky and Luria were alert to both possible pitfalls and the need for concrete scientific research. In 1927 they began to consider the question of development in the fields of evolution, history, and ontogeny. This resulted in the only substantial work of theirs that was published at the time, namely *Studies in the History of Behaviour: Ape, Primitive, Child* (Vygotsky and Luria 1930a). This reviewed Wolfgang Koehler’s studies on the thought processes of chimpanzees and, more critically, the writings of Lucien Levy-Bruhl and Richard Thurnwald on differences in the way people think in different forms of society. Finally, it looked at current theories of child development in the light of Vygotsky and Luria’s own ideas and research. By the time it was published Vygotsky’s own work was already in the process of reaching a substantially higher level in the field of the development of higher mental functions, but remained unpublished (Vygotsky 1997h). Both Luria and Vygotsky looked forward to undertaking serious cross-cultural research of their own.

Cultural-historical theory and its implications are rarely looked at as a whole. Following Vygotsky and Luria's demands for concrete empirical investigations, it has been assessed in terms of these constituents, namely child development, comparative psychology, with cross-cultural studies as a possibly dubious element, probably best left to anthropologists. We have, however, seen previously that, in addition to the examination of research into the mental development of apes, 'primitive' men and children, Luria examined the neuropsychological implications of psychological development. Generally speaking Luria's findings are omitted from the whole discussion. To me they are central to it, and possibly represent the most successful part of it. But, since we have spent the major part of this thesis on that question, we will look at cultural-historical theory as a whole.

How did it ever come to be seen as a whole? What sense of logic informed it? Was it simply the result of evaluating, sifting, and creating a series of investigations that could disprove or build on the work of such Western writers as Koehler, Levy-Bruhl, Thurnwald, Werner, Piaget and so forth? It is undeniable that their work played a crucial role in stimulating the development of the theory (cf. e.g. Van der Veer and Valsiner 1991).

But it is also impossible to ignore the fact that at no other time in their careers did Vygotsky and Luria rely so much on the works of Marx and Engels to stimulate their theoretical ideas, and they clearly saw their work as Marxist. It was by no means an attempt to simply create a 'Marxist psychology,' and the criticisms of their alleged reliance on 'bourgeois theorists' in Stalinist Russia show that this use of Marxism was no mere attempt to placate the authorities. But they – particularly Vygotsky – did creatively develop the ideas of Marx and Engels.

Marx noted and distinguished our "natural history" and "historical nature" (Marx & Engels 1976, 39-40). It was the latter that primarily concerned Marx, together with the key role that social production plays within human history. He saw the first humans as dominated by nature and barely distinguishable from other animals, except that they were not ruled by their instincts. Gradually with the increase in population and the division of labour – including that between physical and mental labour – humans change (ibid. 44-5). By "developing their material

production and intercourse, [they] alter, along with their actual world, also their thinking and the products of their thinking” (ibid. 37).

He amplifies these comments in *Capital*:

Labour is first of all a process between man and nature, a process by which man, through his own actions, mediates, regulates and controls the metabolism between himself and nature. He confronts the materials of nature as a force of nature. He sets in motion the natural forces ... Through this movement he acts upon external nature and changes it and in this way he simultaneously changes his own nature. He develops the potentialities slumbering within nature, and subjects the play of its forces to his own sovereign power. We are not dealing with those first instinctive forms of labour which remain on the animal level (Marx 1976, 283).

Vygotsky and Luria’s position is identical. Not only do the products of our thinking change, but also the nature of human thinking - not simply in terms of our view of the world and our relationship to it, but in that the processes of human thinking are added to – by the cultures of the societies in which we live.

Luria, together with A.N. Leont’ev, responded to one misinterpretation (by Jerry Fodor) in the 1970s. “The adult has *various levels of logical thought*, and he can use these levels differently according to his purposes and environmental requirements ...[the same position as held by Levy-Bruhl and Heinz Werner – MH]. A young child does not have these different levels of thought and some theoretical operations are not accessible to the child...”

[But when a child is educated] the acquisition of abstract operations opens new possibilities to thought and results in an immense enrichment in the possibility of finding new relations between concrete objects. This is why we do not believe in the separation of abstract and concrete thinking but – as in Marx’s philosophy – we suppose that a transition from the empirical to the categorical approach provides a new opening in dealing with concrete objects. There are different ways open to this development, and Vygotsky himself mentions that the acquisition of empirical and scientific concepts has different psychological mechanisms (Leontiev & Luria 1972, 314-5).

In the preface to the *Studies*, Vygotsky and Luria explained that in differentiating the three principal lines of development - phylogeny, history and ontogeny, they were not attempting to explain the entire range of behaviour of anthropoid apes, primitive humans or children. Instead they selected “only *a single dominant feature or single aspect of behaviour*”. Furthermore, “in each study, therefore, we have identified a single essential component that has served as a link connecting a given stage of development of behaviour with the very next new stage of development” (Vygotsky and Luria 1930a, xi). This had the drawback of failing to show how these various levels related within adults. But, since the concrete is subject to many determinations, Vygotsky followed Marx’s approach of ascending to the concrete via the abstract. This seems to me to be an unavoidable procedure in such a complex and dynamic set of processes.

After drawing a line under the thinking of chimpanzees in his commentary on Koehler, Vygotsky then tried to set baselines for human thinking – both historical and ontogenetic. In both of these, of course, language was central, though not necessarily the embodiment of ‘reason’. These baselines derived from his own studies and those of Werner into thinking in ‘complexes’ in child development and from Levy-Bruhl’s concept of ‘participatory’ thinking in so-called ‘primitive’ societies. Participatory or ‘complexive’ thinking is thinking seen not in the accepted formal categories of today, but using associations often idiosyncratically placed within so-called ‘families’ - as opposed to abstraction.

On the one hand, it could be argued that the sum total of all known historically developed cognitive processes – reading, for example - must fall within the same set as all known personal cognitive developments. On the other hand, to draw a continuum and label one end as ‘primitive’ and the other as ‘cultured’ could be seen as both judgmental and abstracted from concrete situations. The following comment of Vygotsky seems to fit the judgmental interpretation. With respect to learning disability he wrote, “The meaning of the concept of primitivism is defined by its opposite – acculturation, ... *primitiveness* is the polar opposite of *cultural development*” (Vygotsky 1993, 43).

*If one were to apply this definition to ‘primitive’ peoples it would imply that they are at the ‘primary’ or ‘natural’ stage and that their culture has developed no new cognitive processes. In the *Studies*, however, Vygotsky states, “The primitive human in the strict sense of the word ‘primitive’ exists nowhere today. The human type as represented among [existing] primeval peoples may only be termed relatively primitive”. Moreover, “a psychology of the primitive human has not been founded yet,” (Vygotsky & Luria 1993, 68).*

Vygotsky accepted that people in societies conventionally labelled ‘primitive’ by Western writers did have a ‘practical’ intellect. He also rejected the view that such people were ‘pre-logical,’ instead holding that their logic was based on different complex-based premises, as opposed to logic based on syllogistic reasoning and its associated categories. He also accepted that, because such societies adapted to different environments and priorities than ours, they could outdo us in different forms of activity. It is well known in anthropology that pre-capitalist, and even pre-agricultural societies have despised the mode of life and social values of many allegedly ‘advanced’ forms of society. Vygotsky himself recognized that such changes often involved losing previous creative cognitive approaches. In an unpublished response to a reviewer’s misrepresentations of the views expressed in the *Studies*, Vygotsky reiterated, “‘Primitive man’ is at the lowest level of cultural development. But this is merely the conventional usage. ‘Primitive man’ in the strict sense simply does not exist!” (Vygodskaja & Lifanova 1996, 109).

There was a large body of informed opinion in Russia that agreed to some degree with Levy-Bruhl that there were historical and cultural differences in the conventional forms of thinking. They found to be insufficient the view, espoused for example by W. H. R. Rivers, that people everywhere had always used similar thinking processes. The logical scientific response in such a debate was to go into the field to do the research that would prove the points one was attempting to make. There had been an exploratory expedition to Siberia by educational psychologists (including Zaporozhets) around 1930. Vygotsky and Luria planned theirs.

In 1931 and 1932 Luria led expeditions to Central Asia to remote, mountainous regions where a basically illiterate peasantry, long under semi-feudal and clerical rule, was at long last beginning to obtain some public education. It was also beginning the first stages of agricultural collectivization. Vygotsky and Luria felt that they had to grasp the opportunity to compare cognitive changes in a society in transition. This was easier said than done, as the first reaction of the most remote inhabitants was to flee at the sight of Luria and his team (E. Luria 1994, 61). Many other problems are discussed in Luria's book (1976), but space prevents any elaboration of this here. The expedition's results confirmed everything that Luria and Vygotsky had expected. The dominant practical mode of thinking could not be shaken by suggestions that the peasants try to look at questions in a more categorical or syllogistic way. Only those groups whose lives were in the process of change, who had experienced education, training and/or had begun to learn farm management skills could operate with these new approaches. Subsequent investigations of other societies have confirmed the findings of Luria's studies (cf. e.g. Cole et al. 1971). Of course, the interpretations vary (cf. Cole 1976).

In August 1931 Vygotsky wrote to Luria and praised the success of his investigations into the changing thought processes. Luria's report was a significant event, "the systematic study of systemic relations in historical psychology, in a living phylogenetic process...". He subsequently added:

It has been experimentally demonstrated [by Luria's expedition] – on the basis of phylogenetic material richer than any other ethnological research, more clear cut and faithful than in Levy-Bruhl – that there is a phylogenetic stratum of complexive thinking and, subject to it, another formation, of all the basic systems of the mind, all the principal types of its operations, and – in the long-term perspective – of consciousness itself" (Vygodskaja & Lifanova 1996, 215).

Thus it seems that Vygotsky and Luria achieved their historical baseline for humanity. Vygotsky incorporated these findings in *Thinking and Speech*, where he wrote, "The history of language shows that complexive thinking with all its peculiarities is the very foundation of linguistic development" (Vygotsky 1986, 130). The seeking of this foundation was the occasion of yet further hostile

criticisms. It was not the foundation itself, but the political climate that was the problem.

Unlike Levy-Bruhl, Vygotsky did not limit the scope of complexive thinking to Durkheimian ‘collective representations’ of a society, nor did he link it to a specifically ‘mystical’ approach of such societies. It is used as a distinct *psychological* concept. But to apply the term ‘primitive’, or even ‘primary’, to *historical* processes of adaptation, which necessarily involve rational decision-making in a variety of practical situations is methodologically unacceptable. If, as Leont’ev and Luria said above, the “adult has various levels of logical thought”, these levels must include thinking in complexes or associations. One may speculate that historically, or even phylogenetically, complexive thought might be the primary linguistic form for humans, but one can only prove, or attempt to prove, its validity in child development. Perhaps this is what Luria meant when in 1974 he said, “We obtained a surprising picture from which it became clearly evident that all the categories that we have become accustomed to consider ‘natural’ were in reality ‘social’” (E. Luria 1994, 62). It is possible that he was referring to the investigations of perception that were undertaken, but perhaps he is also referring to modes of thinking. It would be reassuring to think that they were no longer seeking a notional ‘primary’ baseline. On the other hand, Vygotsky and Luria appear to have concluded that complexive thought was found to exist apparently without other levels in these remote villages. This clearly *is* a significant finding. Vygotsky did investigate its forms in child development at some length (1986, chapter 5). Perhaps if the third expedition had gone ahead, Luria would have investigated the various *roles* of complexive thought in *adults*, comparing the groups using it alone with those using other forms in similar situations. Only in this way could a legitimate comparison be made. Luria was, however, banned from undertaking a third expedition as a result of the witch hunt against him (cf. VIII, iii).

The link between what a people thought with its mode of life was a common eighteenth century view (cf. Meek 1976), propounded by the early theorists of capitalism among others and subsequently given a more scientific formulation by Marx. But in addition those influenced by Herder recognized that the *way* people

used language was also affected, but they did not try to link it mechanically with every historical stage in the transformation of production. It should be clear that this *latter* version is what Vygotsky and Luria, as well as Marx and Engels, were considering when they discussed historical developments in cognition. The first recorded forms of writing and new forms of numeracy were undoubtedly introduced at the time of the rise of the first great agricultural civilizations to aid in tax accounting, the promulgation and enforcement of state laws, and the invention of property in land (cf. Goody 1986, 1987). Abstract and categorical concepts and forms of thinking were undoubtedly encouraged by such changes in social organization and new intellectual tools. Many societies in history have followed in their footsteps, including Stalinist Russia. In discussing the changes found in Luria's expeditions and their connections with socio-economic changes it is relatively easy for those unaware of the two versions of the argument to fail to recognize that Luria follows Herder's tradition rather than that of the eighteenth century economists. Also, although relating cognitive changes mechanically to changes in the mode of production may be reductionist, in certain circumstances, as in the growth of those early agricultural civilizations, there may be some validity in this process. Needless to say, in the West this was often misunderstood not only as an attempt to justify Stalin's programme, but also as an attempt to link stages of cognitive development to stages of transformation in modes of production in a rather rigid manner. Luria was certainly forced to use *both* versions when defending himself against Stalinist attacks (cited in E. Luria 1994, 67-8), but Luria's invitation to Wolfgang Koehler to join the 1932 expedition clearly shows his intentions. These are apparent not only in his additional invitations to Lewin, Koffka and Richard Thurnwald (only Koffka was able to come), but also in his statement of the expedition's aims. These included the task of investigating "thinking as a function, subject to historical alterations – namely in the process of the use of speech, interpretation, metaphor and symbol, logical thinking, and so forth" (cited in *ibid.* 64). Similarly, G. F. concludes his attack on Luria by noting that a defence of cultural-historical theory that Luria gave in 1936 maintained "that the sign is the determining factor in the development of the child's psychological activity" (cited in *ibid.* 75).

We turn now to the nature of the cognitive changes, how they are effected or mediated, and their relation to social change. Marx resumes his comments in *Capital*:

A spider conducts operations which resemble those of a weaver, and a bee could put many a human architect to shame by the construction of its honeycomb cells. But what distinguishes the worst architect from the best of bees is that the architect builds the cell in his mind before he constructs it in wax. At the end of every labour process a result emerges which had already been conceived by the worker at the beginning ... he realizes his own purpose. And this is a purpose he is conscious of, it determines the mode of his activity with the rigidity of a law, and he must subordinate his will to it. This subordination is no mere momentary act. ... a purposeful will is required for the entire duration of the work (Marx 1976, 284).

In *The Instrumental Method in Psychology*, Vygotsky referred explicitly to this passage, explaining that his method “seeks to present the history of how the child accomplishes in the process of education what humankind accomplished in the long history of labour, i.e., how he ‘changes his nature’...” (Vygotsky 1981, 147). Marx clearly understood that conscious planning and a purposeful will were mediated by our use of language. He had earlier written that “Language is as old as consciousness, language is practical, real consciousness that exists for other people as well, and only therefore does it exist for me” (Marx and Engels 1976, 43-4).

Herder wrote, “ speech was to a certain degree the prototype of all that followed” (Herder 1989, 356), and Luria (1974, 261) tells us that Vygotsky intended to call his approach ‘instrumental psychology.’ Regarding education Vygotsky wrote, “The first law of development of the structure of higher mental functions ... can be called *the law of the transition from direct, mute, natural forms and methods of behaviour to mediated, artificial mental functions that develop in the process of cultural development*. This transition during ontogenesis corresponds to the process of the historical development of human behaviour...” (Vygotsky 1998, 167-8). Presumably when he realized that others such as Karl Buehler had used the term ‘instrumental psychology’ (Vygotsky and Luria 1994b, 101), he

abandoned the idea to avoid confusion. Another reason was that the term eventually chosen implied recognition of its place among, and relation to, other areas of developmental psychology. Nevertheless, the term ‘instrumental’ appears consistent with Marx, for it does not imply a mechanical relationship to society. On the contrary it involves dialectical relationships – and not only in the mode of life of a given society and the changes that it undergoes in the course of history. The ‘prototype’, language, in particular is not a simple tool. As Herder wrote, “The first sign that I grasp is both a symbol for myself and a communicating symbol for others” (Herder 1993, 21). And both Marx and Vygotsky understood the social and dialogical nature of language, and its role in personal consciousness, as well as its role in conscious activity. Many, however, fail to grasp this point – for example, proponents of the Russian school of activity theory associated with S. L. Rubinshtein. He and his students criticized Vygotsky and Luria’s emphasis on the role of language as a non-Marxist turn to technological determinism (Rahmani 1973, 45). He had clearly failed to notice that they were following in the footsteps not only of Herder, Humboldt, and the Ukrainian A. A. Potebnia, but also of Marx and Engels.

The Part played by Labour in the Transition from Ape to Man, (an unfinished introduction by Engels to a work on slave labour), was frequently cited by Vygotsky and Luria. Today Engels would probably reformulate his suggestion that proto-humans became social because of their need to work co-operatively, which in turn led them to develop the need for language. Although Engels was aware that our nearest evolutionary relatives are social, the weight of this evidence is now such that a necessity driven by co-operative work as something distinct from social life is no longer called for. Furthermore, since it is in childhood, not adulthood, that humans learn to speak, it no longer appears likely. It has been suggested (Kain 1986, 82) that, unlike Marx, Engels was attempting to explain biological evolution in a *historically* materialist way, i.e., by stressing the effect of the forces and relations of production on thinking processes. While it is true that some unreflecting Marxists seem by default to take this view, and perhaps even approve of it, this should by no means prevent scientists from investigating the activities of proto-humans in relation to their cognitive development. It is equally important to recognize Engels’ concerns as evidence of the importance he

attached to linking language to consciousness, and of linking both to human activity.

From the sequence of the article it is clear that Engels, like Marx, recognized that only after the acquisition of language did real human labour begin (Engels 1987, 455, 457). This, together with his comments on the cognitive abilities of animals, was strongly endorsed by Vygotsky and Luria. They used the comments on animal thinking to support their view that thinking is qualitatively transformed when linked to language as a cultural-historical creation. Engels also wrote what might be taken for an introduction to cultural-historical theory.

The effect on labour and speech of the development of the brain and its attendant senses, of the increasing clarity of consciousness, power of abstraction and conclusion, gave both labour and speech an ever-renewed impulse to further development. This development did not reach its conclusion when man finally became distinct from the ape, but on the whole made further powerful progress, its degree and direction varying among different peoples and at different times, and here and there even being interrupted by local or temporary regression. This further development has been strongly urged forward on the one hand, and guided along more definite directions on the other, by a new element which came into play with the appearance of fully-fledged man, namely [human] *society* (ibid. 456). [That is,] by the combined functioning of hands, speech organs and brain, not only in each individual but also in society, humans became capable of executing more and more complicated operations, and were able to set themselves, and achieve, higher and higher aims (ibid. 458).

Engels continued, noting that humans not only change the environment, but also *master* it. With our greater understanding of it we may even be able to avoid the ecological disasters we provoke, especially if we replace production based on the profit motive with production based, not on exploitation, but on working co-operatively and in harmony with nature (ibid. 460-1).

Marx and Engels clearly stand in a tradition that supports the significance of socially mediated forms of communication and analysis for cognitive psychology, and see language and other sign systems as mediating and facilitating cognitive

development. Changes to and inventions of such systems are in turn the result of attempts to adapt to, or control, various natural conditions, and to organize the various forms of production, together with the societies involved in this ongoing relationship between humans and nature (see Goody 1987). These cognitive developments also allow humans to organize major areas of their lives voluntarily, but as Vygotsky said, a child's acquisition of language is not like putting on a new set of clothes. Rather it totally transforms our mental operations. In *Tool and Sign in Child Development*, he and Luria made it plain that they did not accept that the structure of higher mental processes "is invented and discovered by the child...". Nor did they accept "that symbolization is the primary and irreducible *facultas signatrix*, a part of human consciousness capable from the beginning of creating and comprehending symbols" - an *a priori* conception of higher psychological functions associated with Ernst Cassirer (Vygotsky and Luria 1994b, 147.).

Rubinshtein, who condemned Vygotsky and Luria for the technological determinism of which they were obviously innocent, had also been, with Cassirer, a student of the neo-Kantians Hermann Cohen and Paul Natorp. He, however, reacted against Kant's position that, although it was possible to be a "knowing subject", one could not simultaneously be an active participant. Kant's supporters, Fichte and Schiller, adopted as a corrective to this an equally metaphysical position, namely that one only realizes one's essence through one's activity. Rubinshtein seems to have confined his principal psychological concerns to activity, the area most closely related to their primary epistemological concerns (cf. Rubinshtein 1989, 15-16). He took the view that language was simply a medium and not a form of activity – there could not therefore be any such thing as a speech-act. His response to Luria and Vygotsky was that consciousness *is* merely cloaked in words. In Rubinshtein the crucial mediating and facilitating parts of the process – social interaction, language, culture, ideology were peripheral to his theory (cf. Rubinstein 1946). In short, he failed to recognize the dialectical unity of the processes in which languages partake. Other exponents of the activity theory, such as Leont'ev, certainly used it as political cover, and thought that an extension of Vygotsky's work into less controversial areas was also theoretically valid, but Rubinshtein seems to have been the only one to have developed it from an initially idealist philosophical approach. The scientific

research carried out by the adherents of this school was, however far from negligible (cf. the contributions in Wertsch 1981).

In conclusion, the methodology of cultural-historical theory has the virtue of integrating many areas of study and by explaining the nature of their mutual interaction limiting the scope for single factor forms of reductionism in psychology. The alternatives mentioned here: the biogenetic law, abstract generalizations that claimed to explain everything, and some aspects of the work of Rubinshtein and Cassirer are all ultimately reductionist and/or metaphysical. Cultural-historical theory was repressed before it could iron out its inconsistencies. It has returned, and in part been absorbed into our thinking, because it belongs to one of the great non-reductionist traditions, that stemming from Herder. For him understanding was neither *a priori*, nor an added faculty, but developed on the basis of social communication and experience.

Luria himself applied the approach to neurology in the application of his concept of functional systems. This functional systems approach seems in retrospect to be the perfect way to account for and include all the influences to which the systems are subject, and thus to avoid reductionism. This is crucially so, in the unification of the new cultural-historical forms of mental processing with the phylogenetic and ontogenetic formations in the brain. It is a model example of the application of dialectics. In comparison with Rubinshtein's separation of language and activity, we can turn to Vygotsky and Luria's account of the changes they found in functional systems linking speech and action during the child's transition from a situation where speech reflects or accompanies activity to one where it becomes involved in its planning.

This change consists in the fact that *the child's speech, which previously accompanied its activity and reflected its vicissitudes..., moves more and more to the turning and starting points of the process, beginning thus to precede action and throw light on the conceived but as yet unrealized action.* [Hence] this displacement signifies not only the temporary transfer of speech as related to action, but also the transfer of the entire system's functional centre (Vygotsky and Luria 1994b, 120).

Here is an example of how children may eventually develop the various psychological tools at their disposal in order to be able to voluntarily organize their own behaviour. A vivid example of how functional systems may vary between and within cultures is the relatively recent work in Japan, which shows that those using the traditional logographic writing system are vulnerable to different areas of brain injury than those using the phonetic system (Goody 1987, 249-250). Not only are different areas of the brain involved, but different instrumental activities are used to achieve similar ends. Both are indubitably cultural-historical inventions. This is how humans have continued to develop after the initial biological evolution of our species.

III Epilogue

After the second expedition to Central Asia in 1932 the Control Commission of the Moscow Workers and Peasants Inspectorate investigated the work of the Institute of Psychology. Criticisms of Luria's expeditions had been made, but he had published nothing. Nor had Luria prepared anything - for he still had to process and assess all the reports. Nevertheless the commission accepted all the criticisms. The aim was basically to complete the destruction of Vygotsky's research team, make support of cultural-historical theory unacceptable, and intimidate independent-minded psychologists. This episode was merely part of a widespread process. Here it was achieved by closing down the team's meeting place and Luria's last foothold in the Institute, namely his experimental laboratory.

The commission's report does not appear to have been published, but Elena Luria implies that a 1934 article contained the substance of it (Razmyslov 2000). Basically it seems to have accused Luria's work of showing a racist, colonialist attitude towards the people of Central Asia. There is no truth in this. Perhaps the authorities were concerned that, at a time when Stalin was not far from declaring that Russia had attained socialism, Luria was drawing attention to societies that were economically, socially and politically barely reformed. But I would not put

money on the authorities demonstrating any form of ‘sensitivity’ in this period. As mentioned in the preface, Luria had to keep a low profile in 1933-4 and was concerned that without an employer prepared to even partially defend him against the commission, he might be arrested even in Kharkov.

At the very time when the problem of the separation of psychology into discrete fields with discrete methodologies seemed on the point of being largely resolved by the work of Vygotsky and Luria, their ideas were being maliciously misrepresented, attacked, and on the point of being banned. Neither Luria nor Vygotsky were about to rest on their laurels, since even their friends held alternative views and did not recognize the extent of their achievements. Luria and Vygotsky’s research into the fields of ‘psychoneurology’ and medicine and the extensive retraining that they undertook at this time are clear evidence that they themselves considered that they had at last unlocked the gate to new and wider fields of work.

In October 1933 Luria obtained some work at the Medico-Genetic Institute in Moscow, run by Solomon Levit. Here there was an enormous programme comparing twins educated under differing circumstances. In March 1934 Luria returned to live in Moscow, being somewhat fearful of his management’s attitude in Kharkov. In October 1934 he also obtained work at the Institute of Experimental Medicine. But recent experiences had taken their toll. In May 1933 he wrote of having aged considerably during the past two to three years (E. Luria 1994, 80). In the summer of 1933 he married Lana Pimenovna Linchina who worked at the endocrinology laboratory at Moscow University. She proved to be of enormous support to Luria in all his trials. One that was not political was the unexpected death of Vygotsky in June 1934.

Luria found his work with A. N. Mirenova and others at Levit’s institute to be stimulating. But in July 1936 the state issued a decree attacking ‘pedology’, i.e., educational psychology (Wortis 1950, 242-5). Zalkind died of a heart attack when he read it. A vociferous campaign against Levit and his associates, including Luria, was whipped up (E. Luria 1994, 74-5). In December 1936 Luria resigned his two posts and became a full-time student at the Moscow Institute of Medicine.

This is what saved his life (cf. Preface, 9). While purges and famine rocked Russia, Luria at last found a situation where he was both forgotten by his enemies and could also work productively, something that had not happened in six years. He could now begin to apply the functional systems approach in neurology in what he described as the most fruitful two years of his life (E. Luria 1994, 89). But that is another story.

This thesis has, I hope, clarified the elements involved in Luria's theoretical development in the period 1921-1936 and demonstrated the enormous strides made by psychological theory as a result of his and Vygotsky's work. It is true that this work was the end result of an international effort, but the particular steps that Luria and Vygotsky took amounted to a resolution of the 'crisis in psychology' and the potential unification of the discipline.

Appendix I

The influence of Herder's approach to language, human nature, dialectics and cultural-historical theory on Marx – a contribution on their relevance for the study of psychology and philosophy in the works of Vygotsky and Luria ¹

The works of Herder played an important role in the development of Vygotsky and Luria, especially in the late 1920s. Although it is accepted that Marx wrote about language, it has never been stated that he actually supported a particular theory of language. This is not altogether surprising since few eighteenth century theories of language would be recognized as such today. Although Marx's comments on language are not extensive, they are consistent enough to be recognized as being based on the writings of Johann Gottfried Herder (1744-1803). Marx's views thus belong to a great and varied European tradition of thinking about both language and human nature.

In common with most other eighteenth century writings on language those of Herder contain a view of human nature, which in turn is linked to both a psychology and a philosophy. He should be seen as a founder of the genetic approach to psychology and cognition, and also as a precursor of the 'cultural-historical' school developed by the Russian psychologists Lev Vygotsky and Alexander Luria in the late 1920s.

Part I concentrates on showing how Herder's writings on language and human nature influenced Marx. Part II looks at how Herder's philosophy can be seen in the context of the history of the methodology of materialist dialectics, and is more exploratory. It is all too common to restrict Marx's sources to Smith, Ricardo, Hegel, Feuerbach, and the French socialists. Adding new elements to these helps to place Marx and his sources in a wider historical perspective – as he himself would have expected. I hope it will allow a clearer perspective on the relevance of Marx's writings in the fields of language, psychology, and also philosophy – all areas of crucial importance in the work of Vygotsky and Luria.

It thereby permits the examination of these writings, and their role in the work of Vygotsky and Luria, in a way that does not necessarily assume that they were used for purposes other than their own scientific work. Some might consider in retrospect that Vygotsky and Luria were attempting to articulate the psychological and neuropsychological mechanisms of such a model. In so far as this model proposes that humans are not simple mechanisms, but may be able to consciously make their own history, there is a strong element of truth in this. Indeed this appendix aims to demonstrate its relevance for Vygotsky and Luria's work, though as the main text demonstrates, it was some time before it was possible for them to raise such questions in a theoretical form, or indeed a practical form. Where it is relevant to the development of their work, I have referred to this appendix in the main text, and it should be treated as a historical and philosophical companion to the main text. It can also be used in our assessment of their work, and in understanding their development of a theoretical tradition. As far as I am aware, no attempt to do this deals with the aspects considered here. A comprehensive approach to this lies beyond the scope of this thesis, but the lack of a discussion of these questions needs to be addressed.

I

The development of individuals, like the history of humans in general – as of nature as a whole, is what Herder called “a theatre of transformations” (Herder 1969b, 283). Human understanding is neither a priori, nor an added faculty, but has to be developed in society on the basis of communication and experience. In order to think, then act, consciously we need first to isolate, focus on, and analyse individual moments within a stream of impressions. Language makes this possible through the use of arbitrary signs. Sound can be employed sequentially to convey information and explain processes. Through speech, hearing becomes a fulcrum for the other senses and for the development of understanding. “This most difficult of arts, speech, was to a certain degree the prototype of all that followed” (Herder 1989, 356, cf. Herder 1969b, 314), and humans must develop their arts for they “must learn everything” (Herder 1969b, 314-5).

The mind, for Herder, is a developing unity within a developing active social individual. To attempt to understand this simply in terms of the ‘materialist’ philosophies of rationalism, or sensualism, or the psychologies (such as they were) of faculty theory and associationism was nonsense. Herder insisted on “the integral unity of sensibility, reason, impulse, and ethical will” (cited in Schuetze 1925, 536). As Schuetze wrote, “the four crucial conceptions of modern humanism, namely: genetic history, biological growth, the social character of man, and the integral active unity of the three within individual personality, received their essential meanings at the hands of Herder” (ibid. 549).

There is absolutely no evidence at all that Vygotsky and Luria read Herder, though the latter’s views on language and human nature were at least partly disseminated in Russia – though mediated to some extent by the views of Humboldt - through a writer whom Vygotsky did read seriously, namely A. A. Potebnia (1835-1891). The title of Vygotsky’s book, *Thinking and Speech* (Vygotsky 1986), must be seen as a comment on Potebnia’s *Thought and Language*. Indeed, Nadia Kerecuk has alleged that Vygotsky’s ideas on children’s acquisition and development of speech were largely borrowed from Potebnia (Kerecuk 2001, personal communication). Hopefully her forthcoming translations of Potebnia will clarify this. It bears repeating, though, that the approaches adopted by Vygotsky and Luria needed experimental formulation and verification. Vygotsky and Luria also read the works of Karl Marx, whose views on language and consciousness were taken *directly* from Herder. Since this connection has never been seriously envisaged, and it also to some extent involves a reassessment of both Herder and Marx, this essay concentrates on demonstrating it in respect of those elements that I consider relevant.

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Marx attempted to settle accounts with his philosophical background in *The German Ideology* (1846), part I of which was first published in Russian in Moscow in 1924. (It was cited almost immediately by Vygotsky – see below). Here Marx also sketched out the first version of *his* materialist conception of history and, *in*

the midst of this, made his most extensive comments on human nature, language, and consciousness. He took the latter straight from the works of Herder, of which he possessed a collection (Marx and Engels 1985, 265).

Because of the complex and unfinished nature of this part of *The German Ideology* I have singled out the points taken from Herder and have added Herderian points from elsewhere in Marx's writings. The references are from his *Essay on the Origin of Language* (1772) and *Ideas for a Philosophy of the History of Humankind* (1784-91). It is important to note that generally speaking Herder's positions are easily distinguished from those of his predecessors and contemporaries, such as Locke, Condillac and Rousseau, and that the following points could *not* derive from other sources ².

1) Language is material. "The mind is from the outset afflicted with the curse of being 'burdened' with matter, which makes its appearance in the form of agitated layers of air, of sounds, of language" (Marx and Engels 1976, 44). In his comments Herder notes that "All humans have ever thought, wanted, done, or will do ... depends on an agitated breath of air" (Herder 1989, 346).

2) Animals specialize, whereas humans generalize and are therefore less dependent on instincts. The early human "is distinguished from sheep only by the fact that with him consciousness takes the place of instinct or that his instinct is a conscious one" (Marx and Engels *ibid.*). Vygotsky noted (1971, 80-1) that, in the light of these comments, psychoanalysis had exaggerated the role of the unconscious. Kurt Goldstein (1939, 478) actually cited Herder in arguing against those who proposed separate, competing levels of the organism. Marx's bald assertion is elaborated more plausibly in Herder's version (Herder 1969b, 264, 268). Emotions too are integrated within human psychology and are not compartmentalised.

3) The clearest connection between Herder and Marx is found in Vygotsky's favourite passage from *Capital*, which we have previously cited. Marx wrote, "A spider conducts operations which resemble those of a weaver, and a bee would put many a human architect to shame by the construction of its honeycomb cells. But

what distinguishes the worst architect from the best of bees is that the architect builds the cell in his mind before he constructs it in wax. At the end of every labour process, a result emerges that had already been conceived of by the worker at the very beginning, hence already existed ideally” (Marx 1976, 284). Although humanity is not personified as an architect in Herder there are *numerous* examples from which Marx almost certainly borrowed both the spider and the bee.

Herder wrote (1993, 21, 1969c, 127-8): “The bee in its hive builds with a wisdom that Egeria could not teach her Numa, but apart from these cells and its pre-ordained business therein, the bee is nothing. The spider weaves with the skill of Minerva, but all its skill is woven into this narrow spider space. That is its world. How marvellous is this insect, and yet how narrow the sphere of its activity”. [Richard Rawles informs me that Ivana Markova cited this passage in the same connection (Markova 1990, 151)].

4) In 1844 Marx wrote, “The animal is immediately one with his activity. It does not distinguish itself from it. It *is its life activity*. Man makes his life activity itself the object of his will and his consciousness. He has conscious life activity. It is not a determination with which he directly merges. Conscious life activity distinguishes man immediately from animal life activity. It is just because of this.... That he is a conscious being, that his own life activity is an object for him. Only because of that is his activity free activity” (Marx and Engels 1975, 276).

Herder wrote (1993, 26, 1969c, 130-1): “If man has conceptual powers which are not limited to a honey cell or cobweb – and which therefore are inferior to the skilful capabilities of animals within those spheres – it is precisely because they thereby acquire a *wider perspective*. Man has no single activity which he cannot improve, he has the opportunity to practise in many spheres and hence always to improve. A thought is not a direct act of nature and hence it can become his own work. If instinct must thus disappear...man thereby obtains increased clarity...he thus becomes independent, can seek a sphere of self-reflection, can mirror himself within himself. No longer an infallible mechanism in the hands of nature, he himself

becomes the purpose and goal of his activity”. And this reorganization of powers leads to humanity’s relative freedom.

Thus far consciousness and language have been largely presented as cognitive phenomena, and as such they empower the species, the group and the individual. Hence they meet many of Marx’s basic theoretical requirements for psychology, because they give an explanation of the qualitative difference of humans, their social life and their labour, and *equally significantly*, the recognition that humans can potentially organize their lives rationally. Marx wrote, “Humans make their own history, but not of their own free will, not under the circumstances they themselves have chosen, but under the given and inherited circumstances with which they are directly confronted” (Marx 1973a, 146). Without a view of human nature that sees humans as capable of changing these conditions, socialism would be inconceivable – as, indeed, would an effective psychology.

5) The following quote leads on to language as a social phenomenon. Here Marx links language directly to consciousness. “Language is as old as consciousness, language *is* practical, real consciousness that exists for other people as well, and only therefore does it also exist for me; language, like consciousness, only arises from the need, the necessity of intercourse with other people”. And “Consciousness is, therefore, from the very beginning a social product and remains so as long as humans exist at all” (Marx and Engels 1976, 44).

Herder wrote (1993, 43, 1969c, 141): “How splendid that this new man-made sense of the spirit also constitutes at its very beginning a means of communication. I cannot think the first human thought, nor form the first reflective judgment, without conversing within my mind, or striving to converse with others (*dialogieren*). The first human thought therefore – in accordance with its nature, is placed in a position to dialogue with others. The first sign that I grasp is both a symbol for myself and a communicating symbol for others”. It is difficult to think of a more ‘Vygotskian’ sentiment!

6) In the *Grundrisse* Marx wrote: “As regards the individual, it is clear that... he relates even language itself *as his own* only as the natural member of a human community. Language as the product of an individual is an impossibility. Language itself is the product of a community, just as it is in another respect the presence of the community” (Marx 1973b, 490).

Herder concluded that same-language communities would be the ideal basis for the social organization of communities, replacing the competing, aggressive and oppressive class-ruled states based on the private ownership of property (Herder 1969b, 303-5, 307, 310). But he wasn't prescriptive about the same language element – his basic criterion was that humans should live and organize themselves in neighbourly “living communities”. His views were later deliberately distorted by German nationalists, but were correctly seen by nineteenth century Slavs as supporting their anti-imperialist struggles – as witnessed in the support of T. G. Masaryk, the leader of the Czechs in 1918.

The last quotation from Marx applies to early societies. It deals a neat blow to the ‘theories’ of Nikolai Marr, whose views dominated Russian linguistics from 1928 to 1950. Marr held that language arose only in class societies as a by-product of production. He even allegedly criticized Engels for stating that humans could speak before the advent of class societies (cf. Chikobava 1950, Thomas 1957). Unfortunately his views became necessary to cite under Stalin's rule. Bakhtin, publishing under the name of his friend Voloshinov, famously used them in 1929 as camouflage to pass the censor. Rather surprisingly, many well-known commentators of very differing persuasions have uncritically adopted these views, and even attempted to marry Bakhtin's views with those of Vygotsky³.

Languages do reflect social and historical divisions and changes, and change themselves in certain ways – e.g. vocabulary, linguistic conventions, stress or otherwise on individuality, predominance of more concrete or more abstract terms or ways of thinking – as Luria discovered in his psychological surveys in Central Asia in 1931 –2 (Luria 1976). This, we have seen, is explicit in Herder and Marx,

although the latter felt that in revolutionary circumstances language lagged behind social change (Marx 1973a, 146-7).

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So far I have abstracted those statements of Marx on language, consciousness and human nature, which demonstrate that his views on language and psychology were taken from Herder. In *The German Ideology* Marx, like Herder, does attack *idealist* theories of consciousness, and when concentrating on historical materialism he naturally stresses the importance of labour and production. But he does accept that production does *not* play a *driving* role in history until the increase of population that accompanied the adoption of agriculture (Marx and Engels 1976, 31, cf. Herder 1969b, 290, 315), and, later, that language and consciousness are preconditions for human labour - a position also held by Engels (Marx 1976, 284; Engels 1987, 455-7). Marx would hardly have made such memorable comments on language and consciousness if he did not consider it a prerequisite for his position.

I began this section by noting the connections to be found in Herder between ideas on language, human nature, psychology and philosophy. It is important to realize that with regard to the first three, any theory purporting to *derive* from Marx cannot get past first base without recognizing that Marx had a humanistic and cognitive approach to language, consciousness and human nature that borrowed much from Herder.

Vygotsky and Luria, after many years' practical work and study, including an intensive examination of Marx's ideas, came to similar conclusions. As a result they were the *only* Marxists / psychologists to develop the ideas of Marx and Herder into significant scientific concepts, and, because of this work, were also praised as major scientists in their own right by their colleagues. I hope it is evident from this thesis that they also saw the centrality of these areas for *any* serious philosophy, Marxist or otherwise. Luria's work recognized language's central place in the reorganization of higher psychological processes, and its consequent

role in many functional systems associated with, for example, social interaction, learning, planning, thinking, consciousness and self-identity. This, in itself, precluded attempts to claim legitimacy for any hypothesis that sought to reduce language theory to a single basic factor or system.

II

There is a dialectical dimension in Herder that precedes Marx. This should already be apparent. It is this that is referred to in Paul Reimann's article portraying Herder as a precursor of Marx in his use of dialectics (Reimann 1929). Although he did not credit him with directly influencing Marx, I think that, now a direct connection between the two *has* been established, this link can be extended. This section examines how far we can *legitimately* extend this link.

Herder's criticism of metaphysics was life long. In his *Essay on Being* (c.1763-4) he pointed out that philosophers could not distinguish so-called "logical" being from so-called "real" being, whereas ordinary people knew what being was and that both it and they themselves existed long before any professional philosophers. But, even accepting the need for abstractions, being could not be theoretically isolated - it could only occur interdependently with time, space and energy. And, since we are part of this, it is legitimate for us to relate to them not as abstractions divorced from ourselves. "Is there no order, no unity in the chaos of unanalysed and unresolved concepts? As long as we treat all matters as purely objective (and to that extent let subjective philosophy drop) we will make no progress. But don't these material principles all have a point of connection in us - well then - we can draw them in". This is not difficult for us, since "being is a concept of practical experience entirely" (Herder 1984, 586, 582).

He called this subjectivism. It is, but only in the sense that we recognize our connections and relative dialectical position within the scheme of things. It should be clear that both Herder and Marx accepted the existence of an *objective* reality, independent of humans, and also rejected subjective psychology. Herder could thus be said to have used "materialist dialectics" *avant le mot*. And in an active sense his

position is also a philosophical *precondition* for the recognition that humans can be *conscious historical subjects*.

Trotsky wrote “Consciousness is a quite original *part* of nature, possessing peculiarities and irregularities that are completely absent from the remaining part of nature. Subjective dialectics must by virtue of this be a distinctive part of objective dialectics - with its own special forms and regularities... ” (Trotsky 1986, 102). Trotsky was talking of cognition here, but this distinctiveness is even more evident if we apply it to an individual’s *growing* comprehension of his or her interrelationships with the world. But there is a sense in which - at certain times associated particularly with revolutionary transformations of society and our interrelationships with nature - humans *collectively* passed through this stage, when they began to see both nature and their own activities as developing processes - concentrated *historical phases* of “subjective dialectics”.

In his oft quoted *Theses on Feuerbach* (1845), Marx wrote, “The chief defect of all previous materialism – that of Feuerbach included – is that things, reality, sensuousness are conceived only in the form of the *object*, or of *contemplation*, but not as *human sensuous activity, practice*, not subjectively. Hence it happened that the *active* side, in contradistinction to materialism, was set forth by idealism – but only abstractly, since, of course idealism does not know sensuous activity as such” (Marx and Engels 1976, 6). If one thinks of Newton’s “mechanical universe” or La Mettrie’s *L’homme machine* (1747) it is easy to take Marx’s comments at face value, but we must also remember that in later years Marx and Engels praised the dialectics of Diderot, and also Rousseau’s *Discourse on the Origin of Inequality* (1755). This provides some recognition that they had previously overstated their case. But nevertheless they show no awareness of Diderot’s writings on evolution. The difficult publishing history of these works may partially explain this lapse, but given that Marx said that Diderot was his favourite prose-writer, one would expect him to have at least commented on the conclusion of *On the Interpretation of Nature* (1753).

Trotsky wrote, “Dialectics is the logic of motion, development, evolution.” And also, “dialectics is the logic of Darwinism ... the logic of Marxism (in opposition to rationalistic, idealistic theories of the historical process), the logic of philosophical materialism (in opposition to Kantianism, etc.)” (Trotsky 1986, 86-7). In respect of evolutionary theories and those who criticized rationalism, idealism and Kant in the second half of the eighteenth century, Marx’s awareness was deficient, and not only in the case of Diderot.

Marx used Herder’s works with regard to language, but in print makes no acknowledgment of either them or Herder’s other contributions. Although not a supporter of evolutionary theories in biology, and, as head of religious education in Weimar, frequently likely to interpolate comments on God, Herder nevertheless conceived historical thinking along evolutionary lines. In *Ideas for a Philosophy of the History of Mankind*, he situated humans firmly within nature and history. He began by writing about the universe, the properties of the solar system, earth, natural history – plants and animals, and finally humans. He saw humans not only as part of nature but also with the potential to operate very differently from other creatures. Humans could use or misuse their rational potential. Humans weren’t innately blessed with “Reason,” nor did history inevitably involve “Progress”. Human potential could only be developed in the course of individual, social, and historical development. In various works he saw the development of both nature and history as inherently involving both evolutionary and revolutionary episodes – the latter arising from contradictions within those processes of development (cf. Knoll 1992).

Herder celebrated the concrete - as one might expect from a poet. He systematically attacked Kant’s abstractions - pure reason, practical reason and judgement - on the grounds that they should not be treated separately, as in the faculty theory of psychology, because they actually entail interactive processes. Certainly Kant sacrificed psychological reality in favour of the logical systems of his epistemology, and his “knowing subject” remains an abstract individual, a ghost of Herder’s real one. For Herder, since all voluntary mental operations are mediated and facilitated by languages - which are neither abstractions, nor

creations of God - but created and developed by humans in concrete historical societies, ideas cannot be treated as if they exist apart from our lives and activities (Herder 1967, 18-9, 63-6, 179; cf. also the quotations in Leventhal 1990).

As we have already seen, in *The German Ideology* Marx uses these ideas extensively. He adds "People are the producers of their conceptions, ideas - that is real active humans as they are conditioned by a definite development of their productive forces ... consciousness can never be anything but conscious being, and the being of people is their real life - process". And "It is not consciousness that determines life, but life that determines consciousness" (Marx and Engels 1976, 36-7).

Marx's introduction of the concepts of "base" and "superstructure" into his work parallels this approach, and is certainly compatible with it. But the approach discussed in the previous paragraphs should be *distinguished as a separate strand* of Marx's ideas, and he himself tends to use different phrases in discussing it such as "mode of life", "life activity", and "life-process", instead of his economic terminology.

When Marx was writing, Herder's version of this approach had already been attacked by Kant and Hegel, and distorted by their followers, and, perhaps because it has never been given a convenient label, it has never received appropriate recognition. Martin Schuetze (1930-1) showed how Kantian interpretations of Herder distorted his views. It helps us understand why Vygotsky and Luria had to return to Herder's perspective in order to escape the Kantian approach of much nineteenth century psychology. Herder's influence on Hegel is accepted, though not always understood (Taylor 1975) ⁴. But Hegel's negative remarks about Herder may explain, as with Rousseau, why Marx failed to acknowledge him. Perhaps more relevant than the negative influence of Hegel, is the fact that, having already dug himself out from under Hegel in his own works, Marx was unlikely to hand the credit to Hegel's predecessor. Also significant is the fact that Marx's access to the works of Herder during his crucial periods of exile was probably

restricted to his own memory of a couple of his works, since his own copies had not been forwarded to him.

Hegel's mystical depiction of the role of consciousness is a reflection of this growing awareness that humans could be historical subjects. But it is *only* from this *real* "*subjective dialectics*", embodied in individuals, communities, and then in the beginnings of workers' movements, and *not* from Hegel's *abstractions*, that Marx conceived the historical role of the working class to liberate humanity. I suspect that Herder's subjective dialectics helped Marx make this transition and break with Feuerbach's ahistorical abstractions, and that this is why Marx used Herder's work in particular in the opening chapters of *The German Ideology*. Marx should have recognized that there were materialist writers who grasped the subjective side. On the other hand, what was lacking in Herder's time was either a unified humanity capable of taking advantage of this, or, from the Marxist perspective, a class potentially capable of representing the interests of humanity as a whole.

It should be noted that dialectical logic may be applied both idealistically and materialistically. It is said that dialectics can become materialist when it is applied to concrete processes. Although Marx clearly used the concept of 'contradiction' (used by Herder as well as Hegel) in *The German Ideology*, it is sometimes said that the influence of dialectics on Marx's epistemology was at its lowest, and that in some senses the latter was largely empiricist from 1846 to 1856 (cf. Kain 1986, chapter 2). Herder argued that an empiricism that failed to recognize the dialectics of everyday life, 'evolution', and human history would remain an unusable abstraction. This subjective dialectical assumption would therefore be a prerequisite for any epistemology (cf. Lenin 1981, 360). Since Marx's position in the *German Ideology* was the same, we can safely assume that the dialectical influence remained strong. This made it possible for Marx to later employ both dialectical logic and abstract categories within these assumptions in his analysis of capital. Although it is not clear exactly how much of Herder Marx read or understood, the implications of what he did read surely contributed, if only in a subliminal way, to his introduction of "materialist dialectics".

Recent reinvestigations of Herder's work have led to the rejection of nearly two centuries of misunderstanding and misrepresentation of his ideas. Herder was as unique a thinker as Marx, and contemporary writers have developed different aspects of his enormously varied thoughts. Many accept that this former student of Kant made the crucial 'epistemological / critical turn' that critically pre-empted Kant's positions in the *Critiques* (cf. the essays in Mueller-Vollmer 1990, Menges et al 1992, Koepke 1990, 1996) ⁵.

At the same time, it can also be said that Herder adhered to an eighteenth century view of creation, prompting Wulf Koepke's description of him as a "conservative revolutionary". Koepke elaborates: Herder's "religious humanism is based on the conviction of *Ganzheit* (totality), which would preclude an arbitrary separation of nature and the realm of freedom. The creation is indivisible". At the same time, because Herder saw all life as conforming to universal laws, he excluded divine intervention (Koepke 1987, 77, 58). This allowed him both to see unity in diversity and thus avoid reductionism, and to accept determinism and, within that, a realm of freedom. It embodied a dialectical approach to nature, history and psychology that, although consistent with Herder's religious ideas, was genuinely rooted both in "human sensuous activity, practice" and materialism.

Recent writers have naturally used Herder's positions to attack postmodernist positions. As I have pointed out Herder, the language theorist, grounded his epistemology not on language, perception, or cognition, but on natural social being, linked inseparably with space, time and energy. As we are part of all this, we are not passive observers in a universe befogged by discourse, or unable to see the "thing-in-itself" for real, because we only have use of limited senses and perceptions. On the contrary we may be active historical agents operating with the knowledge that we live in a material and dialectical universe ⁶.

In Russia, particularly after the overthrow of tsarism, it was strongly believed that it was possible to improve humans in many ways, as evidenced by Bauer's book (1952), titled *The New Man In Soviet Psychology*. That it was not simply a product

of Stalinist propaganda (though it was that as well) is shown by some of Vygotsky's comments and a 1930 article entitled *The Socialist Alteration of Man* (Vygotsky 1994b). Indeed, in *Educational Psychology* (1926) Vygotsky cited at length Trotsky's version of this view. Alexander Etkind pointed out that the passage attributed to Vygotsky (cf. Van der Veer and Valsiner 1991, 55-6) is actually part of the conclusion to *Literature and Revolution* (Trotsky 1991, 282-3).

Herder's approach to history shows both major advances and limitations. Together with Vico, he was among the first to recognize distinct societies and forms of society in history. Herder also accepted other cultures and other values, notably those of non-European peoples, i.e., he opposed a Eurocentric view of history. He followed Rousseau in blaming the invention of property for humanity's ills. He praised the egalitarianism and freedom from state rule of so-called "primitive" societies – including early European societies. Yet he also accepted, like Rousseau, that however much he detested feudalism and developing capitalism, there was no prospect of a return to such early forms of society.

As we have seen, Herder argued that there was a relationship between a mode of life and the social and individual consciousness of the society based upon it. But he correctly concluded that the formulations of writers such as those of the Scottish Enlightenment and Turgot, who presented the bourgeoisie's "own" version of historical materialism, were simplistic and inadequate. Yet despite the enormous advances that he and others made, they could not grasp the role of production in the dynamics of history (Herder 1969b, 302-4, 309-11, 314-6; cf. also Meek 1976, 192-8, and Meek 1954).

With his versions of subjective dialectics, materialist dialectics, some recognition of the implications of historical materialism, together with his invention and use of the term 'empathy', he was able to envisage humans as actors in history, and in many senses he was one himself. He would thus have proved invaluable to Marx in overcoming Feuerbach's ahistorical approach. But without grasping the role of production in the *dynamics* of history, he could go no further. We should not be

surprised at this. In eighteenth century Germany there was barely a trace of an industrial revolution, let alone an industrial working class. The growth of the latter was an absolute prerequisite for the ideas of Marx himself. With Marx's analysis of the dynamics of history all these various threads could be brought together and we can better comprehend now not only their methodological significance, but also the extent of Marx's achievement in uniting them.

When humans can understand these connections they can consciously attempt to make their own history. Marx also grasped that the workers oppressed under capitalism potentially represented the interests of humanity as a whole. Strangely enough, materialist dialectics are sometimes presented as being applied first to history and then illegitimately transferred to nature. I hope it is clear from this essay that it was applied to humans only because they and their history were recognized as a *part* of nature – as natural processes.

My conclusions are that Marx was directly influenced by Herder's views on language. He also took on the approach of subjective dialectics that reflects Herder's views, but that any influence may not have been direct. It could be argued that if Marx put Hegel on his feet, he was reversing the damage Hegel had done by standing Herder on his head, but with the reward of picking up in addition Hegel's formal dialectical logic. To a certain degree, Diderot and Herder saw their approach almost as common sense, and they didn't use the term 'dialectics' to label their work. The development of dialectical logic encouraged nineteenth century writers to employ it in a more focused manner. Marx was certainly blind to much of Herder - for reasons I have already suggested – and would probably have been amazed (to put it mildly) at the very suggestion that Herder had preceded him in so many ways. I hope this appendix raises awareness of, and clarifies how Marx, and through him Herder, (and probably also Potebnia), were important sources of ideas for Vygotsky and Luria, and help refocus views on some aspects of their work.

Notes

¹. As a prelude to my research into the work of Luria and Vygotsky, I read the classic works of language theory. Herder's ideas could easily be seen not only in the writings of Luria and Vygotsky, but also in the works of Marx. An early version of my ideas exists in an abbreviated and somewhat undeveloped form (Hames 1998). I have checked/ amended the English translations of Herder in conformity with the German. I have often quoted both German and translated works for the additional reason that most of the latter are out of print. I have also checked/ amended the translation of Marx and Engels: *The German Ideology* in conformity with the German (Marx –Engels 1962).

². It is true that versions of points 2 and 4 can be found in Rousseau (1987, 40, 44-5). Herder almost certainly used these, but he transformed the context, putting Rousseau “on his feet” – for Rousseau had assumed that humans were originally virtually isolated with no communication (ibid. 48), whereas, for Herder, humans were social creatures.

³. *Marxism and the Philosophy of Language* was published in 1929 under the name of Bakhtin's friend, V. N. Voloshinov, in order to lower Bakhtin's profile at a time when intellectuals as such were under attack from the state. It amply justifies the description of Bakhtin as “the first Russian postmodernist” (Etkind 1997, 321). There are also similarities both with Althusser's structuralism and Feuerbach's abstract view of human nature: “In so far as I think, I cease to be an individual” (Feuerbach 1973, X, 8-9). Bakhtin brazenly opens by declaring, “To date, there is not yet a single Marxist work on the philosophy of language” (Voloshinov 1986, xiii). He misrepresents and disparages the school to which Marx belongs (Herder, Humboldt, Potebnia). He barely considers psychology to be a science – the individual is merely a structural mediation between the biological and physiological sciences and the “ideological-semiotic superstructure”. He ignores virtually all aspects of individual and social development and the multifunctional role of language in both (cf. ibid. 13, 34-6, 39, 85). He comments that, “as the process of immersion [into verbal communication] proceeds, the child's consciousness is

formed and filled with content” (ibid. 81 n.16). Vygotsky would undoubtedly have extended his wrath at Bukharin’s simile that a human is like a sausage skin filled with social content to Bakhtin’s formulation (cf. main text III, iii, 85 and Vygotsky 1997c, 66). The latter simply refused to accept the concept of what Vygotsky (1986, 228) called “the transition from inter-psychic to intra-psychic functions”. Although it is well known that Bakhtin used Marr’s ideas as camouflage (cf. Voloshinov 1986, 23), I think it is becoming clear that he also used those ideas to sideline everything that was important to Herder, Marx, Vygotsky and Luria – indeed to psychology in general (cf. Hames 2000 ms).

It is possible that Marr on the other hand also had ideas other than his notoriously reductionist ones. Certainly Eisenstein thought so. Indeed, D. B. El’konin made an intriguing statement: “I remember when Luria was unable to get [Vygotsky] to visit Academician Marr when the latter came to Leningrad, although [Luria] wanted terribly to bring [them] together” (cited in Vygotskaia and Lifanova 1999, 3, 38). Later they were introduced by Eisenstein, but unfortunately we learn nothing further.

⁴. By stressing what he terms the “expressivist” approach to self-realization, without linking it to Herder’s materialist agenda – as *distinct* from those of Hegel, Fichte and Schiller – Taylor willy-nilly associates him with idealist philosophers.

⁵. The approach of Morton (1993), and others was stimulated by Wolff’s 1963 study of Kant’s ideas of mental activity. In the context of Herder studies it is easy to read Wolff’s interpretation of Kant (320ff) as suggesting that Herder went further, more effectively than Kant. Mainstream philosophy has yet to address this issue. By and large the similarities between Herder and Marx remain unnoticed. For some East German contributions see Dietze et al 1980. W. Beyer’s article (ibid.), for example, points out Herder’s essentially monistic approach.

⁶. About 30 years ago, *after* completing a History degree, it was a revelation for me to read Trotsky’s writings on Germany in the years prior to Hitler’s rise to power – to realize that humans were not entirely subject to uncontrollable forces, that they have a role to play in their own history, and that it was actually possible

to *predict* the outcome of certain courses of action – i.e., the sectarian policy of the Stalinized German Communist Party, which split the working class and allowed Hitler an easy route to power. History as taught in many schools still does not recognize that Hitler could have been stopped, or that this is even worth discussing. People do make their own history, but they also have the potential to make it consciously and rationally. Contemporary psychology clearly has something to teach such ‘historians’.

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Note: date of first publication for the period before 1936 is given in square brackets.

Abbreviations:

Int. J. of PsA = International Journal of Psychoanalysis

Int. Zts. f. PsA = Internationale Zeitschrift fuer Psychoanalyse

JREEP = Journal of Russian & East European Psychology

SP = Soviet Psychology

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- Note: see also under A. N. Leontiev, M. S. Lebedinsky and L. S. Vygotsky.

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